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# RESULTS OF AN INVESTIGATION,

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# DISEASES.

The all-surrounding heav'n, the vital air,
Is big with death: and tho' the putrid south
Be shut, tho' no convulsive agony
Shake from the deep foundations of the world,
Th' imprison'd plagues, a secret venom oft
Corrupts the air, the water, and the land.
What livid deaths has sad Byzantium seen!
How oft has Cairo, with a mother's woe,
Wept o'er her slaughtered sons, and lonely streets!
Ev'n Albion, girt with less malignant skies,
Albion the poison of the Gods has drank,
And felt the sting of monsters all her own.

ART OF PRESERVING HEALTH, B. III. L. 521.

# RESULTS OF AN INVESTIGATION,

RESPECTING

# EPIDEMIC AND PESTILENTIAL

# DISEASES;

INCLUDING

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CONCERNING

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BY

CHARLES MACLEAN, M.D.

LECTURER ON THE DISEASES OF HOT CLIMATES TO THE HONOURABLE EAST INDIA COMPANY.

---

IN TWO VOLUMES.

*VOL. I.*

London:
PRINTED FOR THOMAS AND GEORGE UNDERWOOD,
32, FLEET-STREET;
*By R. & R. Gilbert, St. John's Square, Clerkenwell.*

1817.

TO

JOHN BEBB, Esq. Chairman,

JAMES PATTISON, Esq. Deputy Chairman,

AND THE

*MEMBERS*

OF THE COURT OF DIRECTORS OF THE HON. EAST INDIA COMPANY.

---

HONOURABLE SIRS,

To whom, so properly as to yourselves, could I have inscribed a work, of which the foundation was laid, and a great part of the superstructure finished, from experience acquired in your service? The additions furnished by my researches in the Levant, can only be considered as a verification, in Plague, of the principles, which I had previously ascertained to be correct, as applied to other acute maladies.

The entire independence, in the treatment of diseases, enjoyed by practitioners in your service, both at sea, and on shore, chiefly in consequence of the superior or-

ganization of your medical establishments, affords facilities to investigation, unknown in any other public service, or in private practice: from whence it happens that the cure, especially in respect to acute maladies, is better understood in India, than perhaps in any other country.

Encouraged by circumstances so favourable to free enquiry, and urged by a profound sense of the inefficacy, in acute diseases, of the dogmas taught at the Universities, and in the medical schools, I was first led to attempt the innovations, which, in a less imperfect form, I now offer as important professional improvements.

I have the honour to be,

Gentlemen,

Your faithful and

Obedient humble servant,

CHARLES MACLEAN.

*London,*
*20th August*, 1817.

# ADVERTISEMENT.

THE object of this work, is, to promulgate principles, respecting epidemic diseases, by the general application of which, independently of considerations of humanity, and of science, the population, and the revenues of states, may be increased; or, which is the same thing, their ordinary waste may be diminished, in a degree, which, *prima facie*, might almost exceed belief.

Together with the elucidation, in a manner wholly original, of the principles of prevention, and cure, of epidemic and pestilential diseases; it involves the refutation of one of the most stupendous errors, that has ever concurred to aggravate the lot of man:

and, in this respect, it may be deemed not an incurious result of my enquiries, that, of all the powers, which are capable of acting upon the living body, *that*, to which, in all Christian states, epidemic and pestilential diseases, have, for the last two hundred and seventy years, almost universally been imputed, is perhaps the only one, which cannot possibly contribute, either to their aggravation, or production.

It is probably not unknown to the public, that, in the year 1815, I employed myself in investigating the Plague, and in treating cases of that malady in the Greek Pest Hospital, near the Seven Towers, at Constantinople.

So peculiar an experience, embracing also that of the progress and cure of the disease, in my own person, will, perhaps, be admitted to confer upon my observations, respecting pestilential affections, a degree of authority, which they could not

derive, either from superior talents, or eminent station.

But I would not be understood from thence to claim any credit for my doctrines, and it were well for science if none were ever accorded, beyond what is due to strict demonstration. All I request of the reader, is, that he will not refuse his assent to my conclusions, unless he can shew them not to be logical inferences; or deny the correctness of my results, until he shall have ascertained, that similar processes, conducted under similar circumstances, do not produce similar effects.

The verification of any result, by a repetition, under similar circumstances, of the processes by which it was originally obtained, is always the best, as it is often the only evidence that can be adduced, of the accuracy of scientific conclusions: and certainly it is the only one, especially in medicine, where so much would otherwise

improperly depend upon the authority of vulgar integrity, or loose observation, that ought to be received.

Such are the proofs of the correctness of the inferences here adduced, which I hold myself pledged, upon any proper occasion, and in a fit manner, to set in full evidence, at the bed-side of the pestiferous patient:— to prove, by the result of contact, under unequivocal circumstances, that epidemic diseases do not depend upon contagion; and, by the effect of the operation of remedies, that I have ascertained the means of cure.

In a work, which, it is hoped, may be of some duration, I have not, for the sake of bulk, knowingly admitted any thing that is trivial; or, for the sake of brevity, omitted any thing that is important: I have, however, every where studied compression, as far as I have thought consistent with perspicuity; and have preferred distinctness to elegance.

In restricting, by the advice of my publishers, this treatise, contrary to my original design, within the limits of two octavo volumes, I have incurred additional labour: Some matter, is in consequence, omitted, which, although to a few readers it might be useful, would, to a great majority, I trust, be only superfluous illustration.

An intention, which I had entertained, of annexing a selection of cases, to exemplify my mode of treating acute diseases, has, for reasons which it is unnecessary to state, been laid aside. But, if the public should appear to desire it, they will be published in a separate volume.

Persons, who may incline to contribute their aid toward dissipating the delusions, or extending a knowledge of the principles, which form the subject of this work, will please to transmit such communications as

they may wish to make, to the care of the publishers, Messrs. Underwood, 32, Fleet-Street, London.

LONDON,
*August*, 1817.

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*** *The errata will appear in Vol. II.*

# PRELIMINARY DISCOURSE.

# PART I.

## PRELIMINARY DISCOURSE.

Epidemic diseases, from the greater number of persons, whom they affect at the same time, their greater severity, and more destructive consequences, are not only intrinsically of more importance than other maladies; but they also possess an adventitious interest, arising from circumstances of a nature altogether singular and extraordinary, by which they are farther distinguished from every other ailment, incidental to the condition of humanity.

It can scarcely be necessary to insist upon the frequency with which every country in the world has had the affliction to behold some portion of its inhabitants miserably perishing, in thousands, of epidemic diseases, not only without being able to afford them the smallest relief, but, in some

countries, in more modern times, without even daring to approach them with assistance.

Thus, not to speak of the Greek and Roman Republics, or of the Eastern and Western Empires, which had only the intrinsic severity of those diseases to contend with [1], Great Britain has seen London, Gibraltar, Malta, the Ionian Islands, and her East and West India Colonies; Austria, Vienna, Hungary, and Transylvania; France, Paris, Marseilles, Lyons, Avignon, Digne, and Toulouse; Russia, Moscow, Moldavia, parts of Poland, Georgia, and the Crimea; Prussia, Dantzig, Elbing, and Thorne; the Minor Northern Nations, their respective territories; Spain, Cadiz, Carthagena, Malaga, and Seville; Portugal, her Provinces both in the Old and in the New World; Naples, her capital city, and the principal towns of Sicily; Tuscany, Florence, and Leghorn; all the other States of Italy, their several dependencies; North America, Philadelphia, Charlestown, and New York; South America, many districts of her fertile and beautiful regions; China, several parts of her extended dominions; and Turkey, almost every portion of her vast empire, made desolate by the ravages of pestilential maladies.

[1] I mean that the adventitious causes of mortality, comprehended in the consequences of the belief in contagion, were unknown to them.

In order to form some general estimate of the probable sum of annual destruction, occasioned by epidemic diseases, throughout the world, let us suppose mankind to consist of one thousand millions[1]; that three in the hundred, being about the usual mortality in the most healthy countries, or thirty millions, die annually, from all diseases; and that one half of this number, or fifteen millions, are occasioned by epidemic maladies.

If we believe, with Hippocrates, that "diseases seldom have any other cause besides the air[2]," and, if we include all diseases, produced by the air, amongst epidemics[3], the mortality here assumed, taken upon an average of the deaths, from this source, amongst civilized and barbarous nations, will not probably be found to be over-rated. But, as, on the one hand, the

[1] Wallace's Dissertation on the Numbers of Mankind in ancient and modern times, p. 10.

[2] Lib. de Affect. Flat. p. 275. Foes.

[3] In sense this is correct, although not in terms. Every disease, which is produced by the air, although, when affecting but one, or a few individuals, it cannot, in strict language, be called an epidemic, which simply means generally prevailing, or affecting many persons at the same time, is notwithstanding precisely of the same nature with the diseases, so denominated, and may properly be comprehended in this view of the subject.

actual mortality may very possibly exceed; so, on the other hand, if it should even fall considerably short of this computation, the subject would not cease to be of extraordinary importance.

The principal considerations, which relate to this, in common with every other class of maladies, resolve themselves into what regards their nature, upon the knowledge of which depends that of the means of cure; and what regards their cause, upon the knowledge of which depends that of the means of prevention.

The reputed nature of epidemic diseases, in as far as the received opinions upon the subject possess any degree of consistency, are shewn to be, every where, different from, or diametrically opposite to their real nature; and the modes of treatment, resulting from them, to be the reverse of the proper means of cure.

Certain opinions, which have arisen in the progress of society, respecting their cause, are, in like manner, proved to be at variance with every logical conclusion; and the means of prevention, flowing from them, to be not simply unavailing, but pernicious.

It is unequivocally ascertained that the doctrine of contagion, as the cause of epidemic diseases, was unknown to the ancient physicians; by whom these maladies were expressly attributed to the air; as well as that the opinion never

entered into the popular superstitions of any Pagan, Mahommedan, or Hindu nation; nor into those of Christian communities, until about the middle of the sixteenth century, when it was first promulgated amongst them, for a certain political purpose, by the head of the Christian Church.

Thus, it appears, that, after having, for two thousand years, implicitly acquiesced in the doctrine of Hippocrates, that epidemic diseases depend upon the air, physicians have, for two hundred and seventy years, no less implicitly acquiesced in the doctrine of Pope Paul III. that they depend upon contagion: and truly, if medicine is to be treated as an affair of authority, it must be admitted, that it can be but of little moment whether the faculty choose to invest the father of Physic, or the successor of St. Peter, with the attribute of infallibility.

Although it cannot, I am sensible, affect the real merits of a doctrine, whether it be shewn to have originated only two hundred and seventy years ago, or to be between two and three thousand years old; yet, since, with a great proportion of mankind, the adoption of an opinion, or the pertinacious adherence to it, is often chiefly determined by the reputed nature of its parentage, or the precise period of its birth, I conceive it

must be a matter of the very first importance, toward extirpating a prejudice, to trace it to its source. To strip error of the adventitious credit, derived from the supposed antiquity, or the fancied integrity, of its origin, appears to be as indispensable a preliminary to the substitution of truth, as it is, in a forest, to clear the ground, before we attempt to cultivate the soil.

The piece of history, which relates to the origin of the doctrine of contagion, constitutes an entire new feature, in the consideration of epidemic diseases.

The nature of this origin satisfactorily explains why the opinion has universally, but exclusively, obtained belief in christian communities; as the direct consequences of the doctrine sufficiently account for the suspension of all enquiry, and the retrogradation of knowledge, in this department.

Hence, also, Quarantines, Lazarettos, and other plague police establishments; of which the commencement is referable to the same æra, and the introduction founded upon the same belief.

Respecting the plague, as it occurs in the Levant, the total ignorance, which prevails, is thus emphatically expressed by a recent writer upon the Turkish empire: "L'experience de tant de

siecles sur la nature de ce mal, se borne donc à la connoissance des symptomes qui l'annoncent, et de ses funestes effets [1]."

A similar observation has, with equal truth, been extended by an ingenious and candid writer, of much experience, amongst ourselves, to epidemic maladies in general: "We know no more of epidemical diseases, or their causes," says he, "than the inhabitants of Soldania [2]."

These authors are only not correct in that they have not gone far enough. Relative to this most important, and most interesting subject of research, involving as it does the fate of millions of human beings annually, our condition is much worse than that of simple ignorance. We have wandered so far into error, that, to recover our lost way has become our first indispensable labour. We must unlearn all that we have been hitherto learning: we must transpierce mountains of consecrated delusion, before we can reenter the right path of investigation.

For two hundred and seventy years, then, the belief in contagion, as the cause of epidemic diseases, and ignorance respecting their means of cure, have, amongst christian communities, served

[1] Tableau General de l'Empire Othoman, par M. D'Ohsson, tom. II. § vi.

[2] A Treatise on Tropical Diseases, by B. Moseley, M. D. 4th ed. p. 638.

mutually to perpetuate, to strengthen, and to extend each other: and, amongst communities of other persuasions, prejudices of a different kind, have concurred to aggravate the evil. In so much, that, I am persuaded, if persons afflicted with pestilence could be abstracted from the influence of the errors, into which mankind have been plunged, in regard to them, and confided solely to nature, or, in other words, to the operation of the surrounding elements, the mortality, which would take place, in consequence of the mere intrinsic severity of those affections, would be comparatively insignificant.

Dividing the whole mortality incidental to epidemic diseases, into sixteen parts, I shall suppose that twelve of these parts, or three fourths, depend upon the consequences of the belief in contagion; one upon the deleterious treatment, where any is employed, founded upon ignorance of the nature of these maladies; and three upon the intrinsic severity of disease, depending upon the proper, or inevitable causes of sickness.

According to this computation, it is obvious, that, by the consequences of the general renunciation of the belief in contagion alone, (including the adoption of means of prevention, founded upon a knowledge of their true cause) twelve sixteenths of all the evils incidental to epidemic

diseases might be obviated; that one sixteenth more might be prevented by the general abandonment of the usual deleterious methods of treatment; and, that, without expecting too much, these evils might be diminished two sixteenths more, by the general adoption, and application, of an efficient method of cure: by all which means, the sum of sickness, mortality, and misery, arising from this source, might be reduced to one sixteenth of its actual amount.

Thus, supposing the maximum of annual mortality, depending upon epidemic diseases, to be fifteen millions, and the minimum five; and presuming that my views of the nature and cure, and of the cause and prevention, of those formidable maladies, may be considered as approaching toward correctness; the smallest annual saving of lives, which would result from the general adoption and application of the principles here promulgated, would be fifteen sixteenths of five millions, and the greatest fifteen sixteenths of fifteen millions, throughout the world; and in a similar proportion in whatever lesser degree they might be applied.

As the calamities occasioned by the consequences of the belief in the doctrine of contagion, are limited to christian communities; so would the proportion of the benefits, which might result from its renunciation.

These benefits may be estimated too high, or too low; perfect accuracy is not pretended to in this computation. But it ought not to be overlooked, that whether it be excessive or deficient, the inaccuracy cannot, in any degree, affect the justness of my reasoning, or conclusions, with respect either to the nature and cure, or to the cause and prevention, of epidemic maladies.

To institute any strict comparison between the degrees of mortality, which occur amongst the inhabitants of Christendom, and those of Mahommedan countries, as connected with the causes of pestilence, is, in the actual state of opinions, impracticable; both because the adventitious causes operate exclusively amongst the former, and because amongst the latter, there are no certain means of computing the deaths. Nor, if the relative degrees of mortality could be precisely ascertained, and if the causes operating were of a nature precisely similar, amongst the inhabitants of different nations, could any inferences be deduced from the knowledge of these facts, in the existing state of ignorance, respecting the means of cure and of prevention, excepting as to the intensity, with which the causes of pestilence might, at different periods, prevail, in these countries respectively.

In the Turkish dominions, however, where, whilst the Mahommedan population is subject to

the influence of the proper causes of pestilence only, the Christian, and especially the Catholic, population, is subject besides to the operation, in various degrees, of many of its adventitious causes, there are data, from which, even without the aid of returns of burials, certain inferences may be deduced, in a general way, respecting the degrees of mortality, which depend upon these two sets of causes. Every one knows, in the Levant, that, amongst the Mahommedan, the devastations, from the plague, are much less considerable, than amongst the christian population. The precise degree, indeed, of this difference, cannot, without registers of mortality, be ascertained. But I remain persuaded, that, whenever such registers shall be kept, if the respective opinions of Christians and Mahommedans, on this subject, should not, in the mean time, undergo any essential change, they will be found to confirm my present inferences; and, that, if, to those consequences of the belief in contagion, which are actually operating amongst the christian inhabitants of the Levant, those other consequences were super-added, of which Turkish wisdom, avarice, prejudice, or policy, does not at present suffer the existence, in that country, as quarantines, lazarettos, and plague police establishments, the extent of the comparative devastation, amongst Christians, would be found to fal

nothing short of the amount, at which it is here estimated.

It is only in those countries, of which the governments are in the hands of Christians, however the population may be composed, that the police regulations, which have arisen in consequence of the prevailing errors respecting the causes of epidemic diseases, are enabled to produce their full measure of mischief.

Although, for a long time, pestilences have been diminishing, both in severity and frequency of recurrence, in countries, which have been advancing in civilization; and, although there is reason to hope, and to expect, that circumstances will never again occur, by which London may have to lament the loss of a hundred thousand, and Marseilles of fifty thousand inhabitants, in the course of a single autumn [1]; yet the numbers that fall victims annually to epidemics, which are considered of inferior degree, as typhus, yellow, jail, hospital, and ship fever, dysentery, &c. even in the best cultivated countries, and under the most favourable circumstances, are still, and probably will long continue, so considerable, that discussions relating to this class of maladies can-

[1] According to the increased population, a pestilent air of similar properties with that of London, in 1665, other circumstances being equal, would now produce double the mortality.

not but be regarded as objects of essential importance to the domestic interests of all parent states; whilst, as connected with navigation, colonies, and commerce, and with the expeditions of fleets and armies to distant or ungenial climes, they are, if possible, entitled to still higher consideration.

Of the extraordinary positive mortality, occasioned by these diseases, of late years, and in countries in different stages of improvement, in various quarters of the globe, there is authentic proof. The devastations which they have committed almost annually in various provinces of Spain; as well as the mortality, which has occurred in some of our own colonies, and during the expeditions of our armies, as shewn by the evidence of official returns, which do not usually magnify calamities, may serve to satisfy the most sceptical mind, respecting the actual extent of this evil, even in situations, in which it is to be presumed, that the unfortunate sufferers enjoy all the benefits that are to be derived from facilities of subsistence, proper attendance, and the application of the medical knowledge of the day.

That this may not remain doubtful, I shall here state a few prominent instances of extraordinary mortality, taken principally from amongst those, which have occurred, subsequent to the publication of my early researches upon this sub-

ject; as well because they mark the æra of the commencement of a system of regular enquiry, concerning epidemic diseases, as because they afford some data for comparison, between the effects of the opinions, which formerly prevailed, and the demonstrated consequences of the adoption of those, which I now, for the second time, promulgate, respecting the nature and cure, and the cause and prevention, of those afflicting maladies.

The first instance of extraordinary mortality, from this cause, to which I shall advert, is the destruction of thirteen, out of twenty thousand, British soldiers, by yellow fever, in a period little exceeding thirty months[1]. Thus, in less than three years, nearly two thirds, not of the sick, but of our whole army, in the West Indies, perished. The idea of contagion generally prevailed; the deleterious practice of blood-letting was copiously employed; and, where mercury was used, it was, both in respect to quantity, and to intervals, misapplied.

In Cadiz, in 1800, out of a population of about 60,000, the number of the affected, from the beginning of August to the first week in November, amounted to 48,520. The mortality was

[1] This happened from 1796 to 1798 inclusive. *Essay on the Malignant Pestilential Fever, &c. by W. Chisholm, M.D. &c. 2d edit. Pref. p. xvii.*

7,387, of which 5,810 were males, and 1,577 females [1].

The fever occurred again in that city in 1804, 1810, and 1813 [2]. In the former year, the mortality was 4,751 [3].

In Seville, in 1800, out of 80,568 inhabitants, 76,488 were attacked with the prevailing fever: 61,718 recovered: 14,685 died [4].

In Xeres, of which the population was estimated at about 35,000, in 1,800, the number of deaths was supposed to exceed ten thousand [5].

In Malaga, the fever of 1803, which commenced in August [6], carried off upwards of seven thousand before the end of November [7]. The mortality of 1804, which was still greater, is included amongst that of the twenty-three cities of Spain, hereafter to be mentioned, which were afflicted the same year.

In Gibraltar, in 1804, out of a population of about ten thousand, there perished, from the 1st of September to the 31st of December inclusive, 5946 [8]; being more than one half of the whole inhabitants. A similar epidemic, but in a slighter

[1] Reports of the Pestilential Disorder of Andalusia, &c. by Sir James Fellowes, M.D. &c. p. 420.

[2] Id. pp. 100, 207 and 237.

[3] Id. p. 21.

[4] Id. p. 421.

[5] Id. p. 440.

[6] Sir James Fellowes, p. 165.

[7] Id. p. 95.

[8] Id. pp. 75 and 449.

degree, prevailed in that garrison, in each of the years 1810, 1813, and 1814[1].

In the southern cities of Spain, in 1804, according to the bills of mortality, 45,889 fell victims to the prevailing epidemic[2]. But the actual mortality throughout Spain, is believed to have been considerably more than double the number acknowledged in these registers[3].

Upon these occasions, all the consequences of the belief in contagion, were inveterately operating. In Gibraltar, "many were found dead in their beds, without an attendant[4]."

Of the practice, Sir James Fellowes, who gives the history of those epidemics at much length, candidly confesses his inability to give any satisfactory account[5]. Judging from the event, we should be obliged to conclude, that, the effect of the treatment employed, in as far as it might have had any influence, must have been pernicious. But the probability is that it had none.

[1] A Practical Account of the Mediterranean Fever, &c. by W. Burnet, M.D. &c. pp. 345 and 352.

[2] Sir James Fellowes, p. 479.

[3] Dr. Bancroft states it at 120,000, (p. 471); and Sir James Fellowes seems to assent to the computation, (p. 171).

[4] Medical and Chir. Review, Vol. XII.

[5] Sir James Fellowes, p. 154.

In 1810, 1811, and 1812, a similar fever again prevailed in Carthagena; and, of those who were seized in 1812, two-thirds died [1].

In Malta, in 1813, during the height of the epidemic, which ravaged that Island, at the rate of ninety in the hundred, or nine in ten, of the sick, died [2]: and of between eighty and ninety, that were sent to the Pest Hospital of the Lazaretto, *only two* survived [3]!

During the expedition from this country to Walcheren, in 1809, of sixteen thousand men, composing the whole British force on the Island, on the 17th of September, 8,200, or more than one half, were sick; on the 22d, 9,500, and, on the 23d, 9,800, or nearly two-thirds [4]. The

[1] Burnet, pp. 232 and 275.

[2] Answers to my Queries, by the Protomedico, or President of the College of Physicians of Malta: Ans. 11.

[3] Of this melancholy fact, I was informed by my friend Robert Grieve, Esq. Deputy Inspector of Hospitals, and chief of the Medical Staff, in Malta, at whose house I was hospitably entertained, during my sojournment in that romantic Island. Should the interests of this worthy gentleman, in his capacity of Superintendant of Quarantine, at Malta, incidentally suffer, it would be one of the very few consequences, which I should have to regret, of the abolition, which I trust, reason will shortly accomplish, of those most pernicious establishments.

[4] Minutes of evidence taken before the House of Commons, respecting the expedition to the Scheldt, Feb. 1810. Returns

sickness continued progressive for some time longer; and the mortality was proportionally great.

In a letter from Sir Gilbert Blane, to Sir Lucas Pepys, dated Middleburg, October 3, 1809, we find a confirmation of this statement in the following words: "It appears from the last general weekly return, that near two thirds of the whole numeral strength of the army is incapable of duty. The mortality, during the last *four weeks*, has been *about a thousand*[1]."

Upon this occasion, a disease, which annually, at the same season, affects a great proportion of the inhabitants of the country, has, by some of the medical faculty, been most unaccountably imputed to contagion[2]: and what the practice was, it is impossible to describe.

of the Dep. Adj. Gen. London Med. Observ. Vol. VII. p. 316.

[1] Official documents, presented to both houses of parliament, in Feb. 1810, letter E. p. 103.

[2] "A great many of the cases which have already occurred, have, I am sorry to say, run into Typhus Fever, &c..... We have, now, therefore, the effects of *contagion*, in addition to climate, to contend with." *Report of Deputy Inspector of Hospitals, F. Burrows, to Lieut. General Sir Eyre Coote, &c. &c. &c.; Med. Obs. Vol. VII. p.* 314. Sir Lucas Pepys terms it "the bilious remittent, and intermittent fever, *liable to degenerate* into Typhus and *contagious* fever, &c." *Letter to the Dep. Sec. at War, Sept.* 14, 1809; *Military Papers, letter E. p.* 52.

In the same year (1809) an epidemic disease attacked the British troops in the Spanish Peninsula: and above a thousand of them died in the months of February and March, immediately upon their return from that country[1]. This disease, obviously occasioned by a long course of fatigue, deficient and irregular nourishment, mental depression produced in the British soldier by the direful necessity of retreating before an enemy, together with other physical and moral causes, quite adequate, even without any aid from a noxious atmosphere, to account for such an effect, has also been, with a similar disregard to rational induction, attributed to contagion; under the denomination of Typhus, a malady, of which, according to the fanciful hypotheses of the schools, the property of contagion forms a principal part of the definition[2].

In endeavouring to maintain the propositions, that Typhus is a contagious disease, and that the disease of the troops that returned from Corunna in 1809, was Typhus, a recent writer contrives to supply us, in the compass of a single fact, with a refutation of doctrines, which he had

[1] Letter of Sir L. Pepys, late Physician General to the Army, to the Secretary at War; Med. Obs. Vol. VII. p. 400.

[2] Typhus Morbus *Contagiosus*, is part of the Nosological definition of Dr. Cullen. *Gen. Morb. p.* 71.

employed a great proportion of eight hundred octavo pages to advocate [1].

The destruction of Hozier's Fleet, in 1726, off the Bastimentos [2]; the loss of 20,000 lives, with Vernon, in 1741, at Carthagena [2]; the fate of the expedition fitted out from Jamaica, for the Spanish main, in 1780 [2]; and many similar disasters, which, with the knowledge that I presume now to exist, might easily have been foreseen and avoided, are all in proof of the great importance, toward the success of armaments, as well as to prevent their destruction, of calculating upon the influence of the seasons, and the laws of epidemics.

The British territories in India, healthy in a degree much beyond what is generally supposed, are however, in some parts, occasionally liable to the miseries of famine and pestilence. In the years 1809, 1810, and 1811, a fever afflicted the provinces of Coimbatore, Madura, Dindigul, and Tinnivelly, by which 106,789 persons perished, besides the ruin of the constitutions of many

[1] *An Essay on the Disease called Yellow Fever, &c. by E. N. Bancroft, M.D. &c. p.* 515. That the variety of the periods from supposed exposure, at which the orderlies and nurses are here stated to have been attacked, is altogether incompatible with contagion, is shewn distinctly in the chapter which treats of that part of the subject.

[2] Moseley, pp. 146, 7, and 163.

thousands. "Such epidemics," say the authors of the report from which this account is taken, "are not uncommon in our Indian dominions; and, as will be seen, have, at different times, been attended with the most unhappy consequences[1]."

Fortunately the belief in contagion does not prevail amongst the Hindus, more than amongst the Mahomedans; otherwise the mortality upon this occasion, would have been increased probably tenfold. As superstition, however, mixes, less or more, with the popular belief, in every country, it appears, that the inhabitants of the districts in question, consider the Pylney mountains as the great cause of all their misfortunes[2].

[1] Medical, Geographical, and Agricultural Report of a Committee appointed by the Madras Government, to enquire into the causes of the Epidemic fever, which prevailed in the provinces of Coimbatore, Madura, Dindigul, and Tinnivelly, during the years 1809, 10, and 11, of which Dr. W. Ainslie was president, and Mr. A. Smith, and Dr. M. Christie, were members. 1816. Advertisement.

[2] Report, ut supra, p. 83. This popular belief is however comparatively of very little injury. And it may be observed, that, from the superior organisation of the Medical department, throughout the British territories, in India, the prejudices of education common to the faculty are much less operative, and the practice, especially in what regards acute diseases, much more efficient, than in any other part of the world.

The inference, which this report supplies, respecting the

In all the instances mentioned, excepting the epidemic of India, and the observation equally applies to pestilential diseases, occurring in christian countries, in general, the belief in the existence of contagion, as the cause of the prevailing malady, has almost invariably obtained; occasioning, besides the superinduction of a

ordinary mortality, in healthy seasons, in that portion of the East India Company's territory, to which it relates, is so curious, that I cannot refrain from stating it. In the district of Madura, according to the return of Mr. Peter the collector (pp. 97 and 98) it is not quite one in sixty: in Dindigul, according to similar returns (p. 96) it is not more than one in eighty-five: in Coimbatore, according to the return of Mr. Garrow, the collector (pp. 94, 95) the *pestilential* mortality was not quite three per cent. per annum, being about the ordinary mortality in healthy seasons in England; from whence we may conclude that the usual mortality of that district, estimating it at one half of the pestilential mortality, does not exceed one in sixty-six. Respecting the ordinary mortality in the Tinnivelly district, which suffered the most from the ravages of the epidemic of 1811, the statement of Mr. Hepburn, the collector, to the Medical Committee (p. 98), does not enable us to form any conclusion. But it is remarkable that the average mortality, in the other three districts, in healthy seasons, does not exceed one in seventy-five; and that, in Coimbatore, an epidemic season is not more destructive to human life, than a healthy one in many parts of Europe.

It would be curious, useful, and interesting, to see similar calculations extended to the different countries of the world.

neglect of all the proper means of prevention and alleviation, founded upon a knowledge of the true cause, a terror, and a resort to detrimental measures of precaution, which never fail to produce a prodigious increase of the otherwise inevitable sickness and mortality.

It also appears, from a strict examination of the modes of treatment, which have been described, as having been pursued, upon each of these occasions, that, too frequently, the least injurious consequences, which could have been expected to arise from them, was, that they should prove simply harmless. These modes of treatment may all be included and characterised under the four following heads: 1. Simply inefficient, from the inertness of the remedies applied, or from the application, in a deficient degree, of agents of adequate power: 2. Indirectly pernicious, from the remedies producing, by their misapplication, new diseases, equally, or more severe, than those which they had been employed to remove: 3. Directly pernicious, from the increase of pre-existing disease, by the positive diminution, or direct abstraction, of the ordinary exciting powers, as the blood: 4. By the preposterous combination of directly debilitating, and directly stimulating means, an effect either noxious, or salutary, but quite uncertain,

must have been produced, according as the former, or the latter, of these agencies, had happened to preponderate; as in the conjoint employment of blood-letting, and mercury. One or other of these descriptions, or combinations of them, will, I conceive, be found to comprehend every species of medical treatment, which does not consist in the due application of principles.

The results, upon all these occasions, both in respect to the prevention, and to the cure, have, accordingly, been such as might be expected, and will always happen, when the state of the atmosphere, the condition of the countries, the circumstances of the population, and the means pursued, are similar.

If such be the ascertained destruction, occasioned by epidemic diseases, even in well cultivated and civilized countries, what must be presumed to be their ravages, in ill cultivated and uncivilised nations, where no means of ascertaining their exact degree of mortality exist? The historical and chronological works, which have appeared in the different countries, which, since the revival of learning, and the discovery of the art of printing, have been most subject to these maladies, are so numerous, that were I even to state their titles, the list would fill a volume of no mean size. I shall, therefore, limit my refer-

ence, to such as are capable of affording useful information, or supplying evidence, upon particular points.

Considering that epidemic diseases are probably more destructive to mankind, than all other diseases, famine, and the sword, combined, it may well seem surprising, that they should not, at all times, have excited a greater, and more general interest, both with governments, and with individuals. In no instance, perhaps, has the inconsequence of our species been more remarkably exemplified, than in the indifference with which they have beheld the mischiefs occasioned by the want of knowledge in this department, and in the neglect of all endeavours to remedy it. If, during the actual ravages of pestilence, the sense of immediate danger never fails to beget desultory efforts towards alleviating the existing evil, it is no less certain, that, the danger being over, the public, as if it could never return, relapse into their wonted security: and, if, since the revival of learning, and the invention of printing, successive epidemics have, in the countries, in which they have occurred, given rise to swarms of *ex-officio* dissertations from the medical faculty, by which the world has been inundated with local publications having only local views; it is also but too true, that no attempt has hitherto been made, upon enlarged views of

science, toward a general, comprehensive, and efficient investigation, of this most important class of maladies.

Beside the causes, which, until the introduction of inductive philosophy, by Lord Bacon, had retarded the progress of science in general, peculiar circumstances have concurred to impede the progress of medical improvement. Of these the principal are the admission of a judicial evidence, in proof of conclusions in science, and the application of a false analogy to the laws of life.

But, amidst the obscurity, which had always enveloped the nature and cure of diseases, epidemics can scarcely be said, till of late years, to have more than shared the common fate of other maladies. It was in consequence of the doctrine of contagion, promulgated a short time previous to the birth of Lord Bacon, that all investigation, respecting pestilential affections, was suspended, and the world forced, upon that subject, to retrograde into false knowledge. The greater intrinsic severity of these diseases; the dread inspired by infection; the influence of the spiritual power, with which the new hypothesis, respecting their cause, originated, and, for the consistency of which it was deemed necessary that it should be maintained; all contributed to confirm, to extend, and to perpetuate the delusion. Reverence for papal authority threw a sanctity over the doc-

trine of contagion; and dread of infection rendered it impossible either to apply known remedies, or to seek new means of cure. Thus, the avenues to knowledge, respecting epidemic diseases, may be said to have been hermetically sealed. The error, which, from political motives, was applied to one epidemic, at Trent, by some called plague, and by others alledged to be scarlet fever, was afterwards, in a similar spirit of delusion, applied to other epidemic diseases, as they arose in succession, and the doctrine now continues universally to prevail, throughout the christian world [1].

It is true, that, since the credit of papal infallibility has been upon the wane, some few writers, struck with the many absurdities, and contradictions, which the opinion involves, have denied the existence of contagion in particular pestilences; and that a very small number have even ventured to call the doctrine in question generally.

[1] In order to procure the translation of the Council of Trent to Bologna, the Pope, and his Legates, as is shewn in the second and third chapters, caused the report of a *contagious epidemic* to be propagated, in order to frighten the fathers of the Council, without having even the pretext of the existence of any actual malady, beyond the common and ordinary diseases of the place and season. In the first chapter, it is shewn, in as far as a negative is capable of probation, that, in respect to epidemic diseases, the idea of contagion had never before been entertained.

But none of these partial, or desultory efforts, have amounted to any thing like a regular investigation, refutation, or proof: and they have never, excepting in one instance, and that only for a short time, been acted upon by any government[1].

The evil has now been long at its height: and no one, I presume, will deny, that, an enquiry, upon a comprehensive scale, into the nature and cure, and the cause and prevention, of epidemic and pestilential diseases, remains a desideratum in medicine. Nor, in entertaining the ambitious, perhaps the presumptuous hope, of being able, in the face of so many formidable obstacles, to elucidate a subject, which has, for ages, been enveloped in seemingly impenetrable mystery, shall I apprehend the censure of the liberal and enlightened mind, even whilst differing diametrically with me in opinion.

Were this a subject of mere idle medical disputation; or, did I know, that any one, less incompetent than myself, was prepared to attempt its elucidation, I should have remained silent. But the share, which it has been my lot to have early taken in the enquiry, and more especially the confirmation, by the results of my recent

[1] The Austrian Government in the reign of the Emperor Joseph II.

experiments on the plague, of the principles which I had previously promulgated, respecting epidemic diseases in general, seem to impose upon me the obligation, in the absence of more able investigators, to impart the fruits of my researches [1].

Nor do I affect to enter upon the discussion of this momentous subject, in the manner of a *coup-de-main;* or, without being fully aware, of the responsibility, which is incurred, by an individual, and of the perils, to which both his interests, and his reputation, are exposed, when he ventures to engage, in a manner single-handed, in so vast and arduous a controversy.

Neither am I ignorant, that, in medicine, of which man himself is the object, innovation of the most trivial kind, or whatever has the appearance of innovation, however true, and even when it militates but little against the prejudices, presumed interests, or self-love, of the faculty, and not at all against those of the public, is naturally received with more hesitation, and distrust, than in in any other department of science.

And if this be the case, of presumed innova-

[1] The task appears to have even become the more incumbent upon me, as, in consequence of the recent unfortunate death of Dr. Valli, I remain the only survivor of those who have attempted a regular practical investigation of epidemic maladies.

tions, that are considered trivial, what opposition may not reasonably be anticipated, to the reception of doctrines, which, not only, are, in themselves, of extraordinary importance, and most extensive applicability, but in direct contradiction to the prejudices, presumed interests, and self-love, of the medical faculty generally; to other private interests, ever active in endeavouring to perpetuate profitable delusions[1]; to the religion of some states, the policy of others, and the avowed sentiments, and consequently the self-

[1] The measures, which would result, as consequences, from the adoption of the principles, which it is my object here to promulgate, instead of deriving countenance from holding forth a prospect of the creation of new offices, or of the increase, or confirmation, of offices already established, beget immediate hostility, by involving the abolition of some, which had previously existed. Thus, a bias arises, antecedent to discussion; and, by some, the question is decided, even before it is agitated. Nor are there instances wanting, of propositions, deeply affecting the health, and lives, of mankind, being submitted, for official decision, to persons, the least capable of judging right, or the most interested in judging wrong, or both. Thus application has been known to be made to the executive agents of the Lazarettos, for information, whether epidemic diseases depend upon contagion, an opinion from which they derive their subsistence! But he, who has an earnest desire to arrive at truth, without an establishment, rather than at an establishment, without truth, will keep "his heart unbiassed, and his mind his own."

love, of almost the whole christian part of mankind[1]!

With the danger of the research after truths, repugnant to established modes of thinking, and the still greater danger of promulgating them, the illustrious Harvey was so much impressed, that, although physician to a monarch, and the

[1] I wish distinctly to state, that, there is, in this work, no question, any where, of religion, as such. The allusion here made is merely to illustrate the influence, which certain circumstances, connected with the politics of the church of Rome, have occasionally had over medical doctrines. In many parts of the continent of *Europe*, this influence has continued down to our own times, by which means, it may be presumed, that some medical truths have been suppressed, or retarded, as we know that some errors have been propagated or prolonged. The venerable Doctor Frank, of *Vienna*, informed me, that, at an early period of life, when he was physician to the archbishop, Elector of *Spires*, he was advised, by that prelate, to suspend the publication of his work upon medical police, lest the freedom with which he treated some matters, strictly connected with his subject, might bring upon him the displeasure of the Church. When we find, that, whilst the belief in contagion, in epidemic diseases, has been almost universal amongst Christians, it has not at all been entertained, either by Pagans, Mahommedans, or Hindoos, it seems not unreasonable to conclude, *prima facie*, that, this difference of belief may have some connection with the difference, if not of their religious persuasions, at least of the policy of their respective churches. It cannot but be a satisfactory coincidence, that history demonstrates this inference to be correct.

member of a privileged order, he hesitated to publish his great discovery of the circulation of the blood, involving as it did but little, in proportion to its intrinsic importance, that could militate against existing interests, prejudices, or passions, lest, says he, all men should be my enemies—ne habeam inimicos omnes homines.

From an undertaking apparently so rash, a friend, to whose opinions I am in general disposed to defer, endeavoured to dissuade me, in words borrowed, with some slight alteration, from a favourite bard:

> Would you from pestilential terrors save a land,
> All fear, none aid you, and few understand.

But after having encountered some danger in the pursuit, it would not, I think, be very consistent, if I were to allow myself to be deterred, by the dread of any responsibility, incident to modern times, from giving publicity to the results of my researches. I consider them, indeed, of much too great an importance to permit that I should, for a moment, hesitate, even if the undertaking *could* be attended with danger, in presenting them to the world. But, to suppose this possible, if my doctrines be true, would be to libel the spirit of the age: and, even if they were erroneous, seeing that it is not unprecedented in medicine to err, I might reasonably

hope for some small portion of indulgence. Confident, however, of the goodness of my cause, I do not desire more than to be judged by a competent and impartial tribunal. Before such a tribunal, and with truth and demonstration on my side, I can entertain no doubt of the victory.

The truth of my principles being fully proved, and the beneficial consequences, which would result from their general application, being rendered apparent, I conceive, that, instead of having either blame, or mischief, to apprehend, from their promulgation, I shall have established a right to call for aid, in the name of humanity, for the furtherance of the great public objects, at which they aim, from every government, and from every people.

Amongst Christians, I am not aware that there is above one insignificant portion, but there is one, which may be presumed to have a motive, arising from direct and invariable interest, for desiring the prolongation of the pernicious delusions, which I would destroy; for

> What damned error, but some brow
> Will bless it?

I mean the executive agents of the Quarantine, Lazaretto, Plague Depot, and Plague Police establishments. But the motives of adhesion,

compounded in whatever proportions of habit and of interest, by which these partisans of contagion must be supposed to be influenced, whilst their own exemption from disease affords a standing demonstration of the fallacy of their doctrines, are too palpable to require a single comment.

But with respect to the disinterested, and otherwise enlightened bona fide advocates of the doctrine of contagion, the case is widely different. And I must here entreat these gentlemen to believe, that, in differing diametrically from so numerous and respectable a body, upon a question of such paramount importance, I cannot but sensibly feel the weight of the responsibility, which I incur; and, that, in combating their opinions, however I may experience pain from the conflict, the respect and regard, which I have been accustomed to entertain for many of them, can undergo no diminution. I find it, indeed, impossible to enter into an unqualified refutation of opinions, which I know to be entertained by men of great experience, consummate wisdom, and much local knowledge, to whom I am also personally under particular obligations, without a strong feeling of anxiety, that I may not, in my zeal, be betrayed into any measure, or expression, that should justly lower me in their esteem.

When I behold, in the ranks of those, whose settled opinions, upon this subject, I deem it imperative upon me to call in question, men of exalted reputation, filling, in those countries most subject to the calamities of pestilence, the highest offices dependent upon the christian governments of Europe, who, by residence, might be presumed to have acquired some personal experience, respecting these formidable maladies; and, from whom, in the course of my investigation of the plague, I had received the most gratifying kindnesses, and the most marked civilities; it can scarcely be necessary to assure the reader, that nothing short of an entire conviction of the truth, and extensive utility of my doctrines, could have induced me to engage in so ungracious an undertaking. Highly thankful for their favours, and feeling the most perfect respect for their characters, I cannot offer to these gentlemen a more appropriate apology for the freedom, with which I have thus presumed to dissent from their long established opinions, than in the language of the good old, though often repeated maxim: "Amicus Plato; Amicus Socrates; sed magis amica veritas." Nor can a better proof be given of the sincerity of this respect, than in the persuasion which I feel, that, upon due proof, they will, upon such a subject,

be forward to evince the magnanimity, which consists in the acknowledgment of error, and amongst the first to rejoice in the refutation even of their own opinions; the more especially as, upon a strict scrutiny, they must be sensible that they are opinions, which they have been led to form, not from their own observation, but from the report of persons, either grossly ignorant, or deeply interested in deceiving them, or both; and consequently resting upon the foundation, not to be recognized in science, of mere faith[1].

To me, as well as to them, the demonstration that epidemic diseases depend upon contagion, if it were true, as the proof would be easy, could not fail to be equally satisfactory; since, as those diseases would not, in that case, be liable to occur more than once in the same person, efficient precautions could always be taken, if not against their introduction, at least against their spreading: a distance of less than five paces, would, according to themselves, confer ample security.

It cannot fail to be felt as an extraordinary, and almost an incredible circumstance, that,

[1] If I abstain from a more distinct specification of the eminent persons, to whom I here allude, it is because I do not consider myself at liberty to represent them as holding opinions, which, however, I have reason to be persuaded that they do entertain them, they have not thought proper publicly to avow.

whilst contagion has been universally assumed, as the cause of epidemic diseases, and narratives of the propagation of infection have been constantly detailed, with as much confidence, as if they were unquestionable truths, it should never have been thought necessary to attempt to establish these alledged facts, upon any ground of direct probation. To prove the existence of a non-entity, we know to be impossible. But, that a non-entity should, for upwards of two centuries and a half, be almost universally taken for granted, in the christian world, and in respect to a matter of science, which is subject to the most ordinary laws of probation, seems to be an instance of credulity almost amounting to infatuation. The cause of this extraordinary phœnomenon, is now, I conceive, rendered manifest. But, so extensive and inveterate has the belief in this doctrine become, and so calamitous, or rather, indeed, destructive, are its consequences to mankind, that, notwithstanding the detection of the origin of the error, it does not appear to be sufficient, that we should plead, in refutation of it, that we are not bound to prove a negative, or that it rests with its advocates to prove the affirmative.

Nor is such a mode of proceeding, although it would be justifiable, necessary; since we are enabled, by the most irresistible combination of

proofs, positive, negative, analogical, circumstantial, and ad absurdum, to demonstrate the impossibility of the affirmative. In disproving the hypothesis of contagion, in epidemic diseases, my conclusions are deduced, much less from data supplied by my own experience, although that has not been limited, than from such as are to be found in the historical records of all ages and of all nations; such as present similar inferences to all unprejudiced minds; and such as, in the aggregate, cannot err. I have also, in part, formed my inductions from a contrast and comparison of the laws, which are common to epidemic diseases, with those, which are common to such general diseases as indisputably depend upon contagion: and from grounds thus incontrovertible, have drawn the plain but inevitable inference, that an epidemic disease cannot depend upon contagion, any more than a contagious disease can depend upon the causes, which produce an epidemic. The demonstration is, I trust, as clear, although not in the same form, as those in the sciences usually denominated exact.

In this class of maladies, besides the higher degrees of pestilence, as plague, typhus, yellow, jail, ship, and hospital fever, are comprehended every affection depending mainly upon the air, of whatever degree, appearance, name, or country.

Contagion being thus disproved, in all these diseases, the injurious consequences of the belief in that doctrine, are, under distinct heads, next investigated: from which it results, if I be not greatly deceived, that, in the whole circle of human opinions, there is not perhaps another individual error to be found, that is productive of so much complicated mischief to mankind. It does not, like errors more strictly medical, affect only the health and lives of the persons subject to its immediate operation; but has besides a distinguished influence upon many of the best interests of society. It affects, to a degree of which it is difficult to form any adequate idea, morals, medicine, commerce, navigation, the intercourse of nations, individual freedom, military operations, the general consumer, and the public revenue. Finally, it occasions a large annual expenditure, by all the nations of European Christendom [1], in maintaining establishments, that essentially contribute to increase mortality, disease, and all the other evils mentioned; and would, if the doctrine which gave rise to them were correct, be still more destructive in their operation [2].

[1] Quarantine, I understand, also exists in America.

[2] If epidemic diseases were contagious, as they are capable of occurring repeatedly in the same person, Quarantines and Lazarettos would be only as so many nuclea of infection; which, from these repositories, would be perpetually

The decision of the question, respecting contagion, is followed, in the third part of this work, by some account of the attempts, toward a regular investigation of epidemic diseases, which have been made by others, since the promulgation of my doctrines upon this subject, in India (I know of none having been made before; for the observations of local practitioners respecting particular epidemics, if even any benefits had resulted from them, cannot be regarded as of this nature); as well as an examination of the results, which have incidentally arisen from the observations of medical officers, attached to armies, upon expeditions to countries liable to pestilential diseases, and from those of medical practitioners, who have treated of particular epidemics, during the same period, in civil life. This portion embraces the years which are past of the nineteenth century. It comprehends the researches of Drs. White and Vaili, and Mr. Von Rosenfeldt in the Levant; those of the medical officers attached to the British and French armies in Egypt, during the first years of this century; and of the civil and military practitioners, who have treated, or written concerning the epidemics, which have recently so often prevailed, in the towns of Spain,

diffused, and consequently could never disappear, while any human being remained alive within the reach of its influence.

and in some of the British colonies, fleets, and armies.

This is succeeded by a narrative of my researches, in the Levant, concerning the plague, in the year 1815.

The obstacles, which there oppose, or rather preclude successful investigation, are fully explained: from which it appears, that, the prejudices of the Turkish government, upon the one hand, and those of the christian inhabitants of the Levant, upon the other hand, constitute a double barrier to investigation, which, in an incipient enquiry, would have been impervious to improvement. Such, indeed, is the nature of these obstacles, that, but for the progress of my previous researches, upon the subject of epidemic diseases, and particularly my experience of their cure, my enquiry, upon that occasion, would, in all probability, have terminated, like those of all my fellow-labourers[*], in the same department, by becoming myself a sacrifice to disease, or some even more imminent peril; or, at least, by not obtaining any useful result.

With all the advantages of my pre-acquired knowledge, I barely found it practicable, in Turkey, at the great hazard of my life, to procure a confirmation of the validity, in plague, of the

* Drs. White and Valli, and Mr. Von Rosenfeldt.

principles, which I had, in other countries, long before, ascertained to be true, as applied to *their* epidemic maladies [1].

For the benefit of my successors, in this line of enquiry, some observations are annexed, on the requisites indispensable to a successful investigation of the plague, in the Levant; which it is necessary that those should strictly attend to, who would combine a chance of personal safety, with researches concerning the nature of pestilence. By a perusal of the narrative of my proceedings in that country, they may become acquainted, beforehand, with the nature of the obstacles, which they will have to overcome, of the dangers which they will have to encounter, and of the stratagems, which they will have to defeat. The path, which, to myself, for want of guides, was both difficult, and dangerous, is rendered, to them, by my experience, both easy and safe.

After having verified, to my entire satisfaction, in the plague, the principles which I had

[1] In distinguishing the epidemic diseases of various countries, I do not mean to infer that they are different in their nature; but merely that they are so modified by the circumstances particular to each country, and quarter of the globe, connected with their causes, as to be distinguishable, in their phœnomena, or symptoms, as their inhabitants are by the joint result of their air, manner, features, complexions, and languages.

previously ascertained to be correct, as applied to other epidemic maladies, I naturally turned my views toward obtaining the means of applying my discoveries extensively to use.

Reflecting that a common life-preserver, or any mechanical invention, for saving a few persons, occasionally from a wreck, is usually deemed, and most assuredly is justly entitled to every attention, and encouragement, I could not have imagined that a proposition contemplating, upon rational grounds, the annual saving of millions of human lives, could have been received, by any government, even that of Turkey, with indifference, or treated with neglect.

I was farther led to hope, since the obstacles to investigation, and to the application of remedies, depending upon the belief in contagion, which are so strong in Christian communities, do not exist among Mahommedans, that the Turkish government might be prevailed upon to establish an institution, at Constantinople, for the purpose of causing my doctrines, respecting epidemic diseases, of which I endeavoured to make them sensible of the benefits, to be applied to general use.

Accordingly, I had no sooner recovered from the Plague, with which I was seized, at the Seven Towers, than I formally proposed to that government, to resume my labours, and to extend

their application, provided they would defray the expence of an establishment, upon a moderate scale, proper for the purpose, and organized after a plan of my own; that of the Greeks having, for reasons which I explained, been found wholly unsuitable.

But, it not only appears not to have been in the policy of the Othoman Porte, to enter into my views of increasing the population of their territory; but, sensible of the odium, which could not fail to attend the rejection of my propositions (for it must not be imagined that the Turks are indifferent to the public opinion of Europe, or insensible of its value) they were extremely anxious, and exerted circuitously all the arts of their diplomacy, to induce me to make the abandonment of the investigation my own act. The snare was not difficult to perceive. And finding that I was firm in my determination, they found themselves compelled to give my propositions a direct refusal.

The representations, proceedings, and correspondence, which took place, upon this occasion, and the pretexts, as well as the real motives, of the Turkish government, for not acceding to my propositions, for repressing the devastations of a calamity, which has been emphatically called the scourge of mankind, as founded upon religious prejudices, inconsequent views of policy,

and a rapacity depending upon the uncertain tenure, by which each successive sultan holds his throne, and his life, are here fully set forth.

This part of the work also contains some matter, respecting the nature and cure, and the cause and prevention, of plague, which will probably be deemed both curious, and new, and which is certainly founded upon a species of personal experience, hitherto, as I believe, without a parallel.

It cannot certainly but be thought singular, by those who believe in contagion, that, after having visited, touched, and administered remedies to all the pestilential patients, in the hospital, I should have almost daily perambulated the streets, and market-places, and entered the coffee-houses, in the great city of Constantinople, along with my interpreter, and on some of the occasions even whilst I was labouring under the disease, without its having been communicated to any person. That the servants of the hospital, after having held the patients in their arms, and dressed their sores, should have daily gone to market, without the smallest restraint, or controul; that they should have sold the clothes in which the patients died; that these clothes, unwashed, should have been exposed to sale in the public bazaars, and again worn by persons in health; that all these circumstances should be

capable of taking place, or rather indeed should have been constantly happening, without any propagation of the disease [1], are facts, which, although certainly true, have never yet been set in full evidence; and for which, whatever the ingenuity of others may prompt them to attempt, or enable them to accomplish, I profess myself wholly unable to account, upon any other ground than the plain and obvious one, that, in this disease, there does not exist a single particle of contagion.

Notwithstanding the ill-concealed indisposition of the Turkish government toward the object of my investigation, and the more active, and dangerous, but not more open hostility, of the executive agents of the pest hospital, the success of my researches, will, if I be not greatly deceived, appear to have been complete, far beyond any former example; or, rather, they

[1] In a season eminently pestilential, although the individual cases of disease could not be more severe (for they could not be more than fatal), their greater number would have rendered it difficult, if not impossible, to have successfully made an experiment of this kind: for, as often as a person after having been within the distance at which contagion is supposed to act, of one labouring under plague, should be affected by the air, the real cause of the disease, the coincidence would be denied, and the malady attributed to contagion.

stand alone, in having, upon this subject, established permanently useful results.

Concerning these infernal depositories (the reader will excuse the epithet; none *above* ground could do justice to the subject) miscalled pest *hospitals,* in the Levant, it is proper that I should here say a few words. Their purpose is to serve as receptacles for all the miserable beings, who are thought fit objects of excommunication, by their families or employers, for having the misfortune to be attacked with the plague. They constitute the intermediate stage to the burying ground; to which these unfortunate victims of barbarity, are, after being almost always despoiled of their property, and often unfairly of their lives, generally in a few days duly transferred. These depots, constituting one of the most direful consequences of the doctrine of contagion, are of course only in use amongst the inhabitants of those persuasions, who entertain that pernicious belief: and their non-adoption by the Mahommedans is one of the circumstances, which contribute to exempt that branch of the Levant population, beyond their neighbours, from the ravages of pestilence. The turpitude of these establishments has not hitherto been properly displayed; and their nature is but inadequately understood even in their immediate neighbourhood. None who enter them, indeed,

are ever expected to return. But persons in health beguile themselves with the idea, that, by the sacrifice of the sick, they are themselves preserved from the malady.—This is an astonishing state of things: and I should have accounted the preservation of my life, upon this occasion, a singularly fortunate circumstance, if its prolongation were to serve no other purpose, than to expose the transcendant villainy, and to elucidate the fatal operation, of the system of those sepulchral abodes.

The narrative of my researches, in the Levant, concludes with a few incidents, which may be thought not irrelevant, of my journey from Constantinople, by the Black Sea, to Vienna.

Subjoined to this part of the work, are the outlines of a plan, which, upon my return to England, I thought fit to present to the administration, with a view to the application of my discoveries, for the benefit of the British Colonies, in the Mediterranean, and parts adjacent: offering to shew, by the result of contact, under unequivocal circumstances, that epidemic diseases do not depend upon contagion; and, by the effect of the application of remedies, that I have ascertained the means of cure. My propositions lie for their consideration [1].

[1] The information which I collected at *Malta*, to be found in the third part of this work, as well as the perusal

Respecting the cause, and means of prevention, of epidemic diseases, the consideration of which constitutes the fourth part of this work, the results of my labours, in re-ascending into the regions of truth and nature, are, in effect,

of the various publications, which have treated of the *Mediterranean* fever, (as the epidemic which has, of late years, so frequently appeared in that quarter, is now baptized,) will serve to convince the discerning reader, that there is, indeed, much need of some such plan, as that which I have proposed, for the application of my discoveries to use. I beg to be understood as not meaning to convey the slightest censure upon the gentlemen at the head of our public departments in those seas. In *Malta*, where I had some opportunities of judging, I observed the greatest zeal, and willingness to adopt such measures for the public safety, as could be shewn to be salutary. And, if measures of a directly contrary tendency were unfortunately pursued, in respect to the epidemic of 1813, it was but in consequence of the unavoidable participation in the general delusion of the age, respecting contagion, on the one hand, and in the deficient state of medical knowledge, respecting the cure of epidemic diseases, on the other. If, previous to the occurrence of that epidemic, I could have spoken, to the constituted authorities of *Malta*, with the same confidence, derived from *practical* knowledge, that I can now do, and had had the opportunity, of laying my proofs fully before them, I am persuaded they would have pursued the measures, resulting from an improved knowledge of epidemic diseases, with as much energy and ardor, as they formerly did the mistaken ones in fashion, believing them to be proper and well-founded.

but a restoration, with modifications, of the general doctrines of Hippocrates.

Since discussions, upon this subject, have, of late years, been opened, with a view to regular enquiry, it has been remarked, as something strange, that Hippocrates should not have once mentioned contagion, throughout his works; as if so obvious a cause of epidemic diseases, had it really existed, could have escaped the observation of that discerning physician. Looking at the matter, as it now appears, in its true light, it would have been much more extraordinary, that the great father of physic, unless he could be supposed to have entertained the unnatural design of destroying his own professional reputation, or to have had some favourite purpose to serve, but which it would have been improper to avow, should have wandered so wide from the plain path of nature, as to have imagined, or passed off such a nonentity upon the world.

The causes, to which epidemic diseases were attributed by Hippocrates, in common with the other eminent physicians of antiquity, were so palpable, and the means of prevention, resulting from the knowledge of them, so obvious, that he could not, we may presume, have imagined that any detailed illustration of them could be necessary: much less could he have anticipated, that the simple, but immutable, laws of nature,

which he deemed it sufficient briefly to expound, were to be lost in the obscurity of the dark ages, or buried under the superstitions of the sixteenth century.

In now demonstrating, therefore, from the nature of things, and by what will perhaps be deemed a superfluity of proof, that it is impossible epidemic diseases should ever depend upon contagion, I merely endeavour to do, what I conceive Hippocrates would himself have done, could he have anticipated that calamities so extensive were to be entailed upon mankind, in consequence of the delusions which were to arise in after ages.

Since, in respect to the cause and prevention of epidemic diseases, modern physicians have shewn so great an alacrity, at the nod of a pretender to infallibility, to depart from the wisdom of the ancients, wandering into the interminable mazes of false knowledge; may it not reasonably be concluded, that, if they have not done so, to a similar extent, in respect to their nature and cure, it is only because, the ancients themselves had not, in regard to these, made near so close an approximation to accurate inferences?

The great Coan, indeed, in glancing at the nature of disease, may be said to have departed less widely from the truth than any of his successors. His "morborum omnium unus et idem est mo-

dus, locus verò ipse eorum differentiam facit[1]," although it does not define what disease is, declares diseases to differ but little from one another. It is a proposition, which borders so nearly upon correctness, that it requires but a very trifling alteration to render it accurate. But, the moderns, as if apprehensive that they could not recede far enough from simplicity, were resolved to constitute every affection of an organ into a separate disease, and to bestow a distinct name upon every distinct cluster of symptoms. Such is the foundation of the absurd fabric of modern nosology!

But, whilst, in respect to the nature and cause of epidemic diseases, which form the subject of my present remarks, to return to the doctrines of Hippocrates, is to approach the majesty of truth, it must be acknowledged, that, in regard to the cure, which is by far the most essential part of medicine, notwithstanding his great and otherwise well-merited reputation, he has transmitted scarcely any thing to after generations, that is worthy of being preserved, or followed. There is, indeed, one part of his practice, as gross and barbarous in its aspect, as it is deleterious in its effects, which it were well for mankind, if it could be consigned to utter and eternal oblivion.

[1] In libro de flatibus.

It is shewn, in that part which treats of the cure, to be as repugnant to reason, as the error of contagion, into which he had escaped falling. But this barbarous part of his practice, is that, in which he has been always most closely imitated, and, of late years, especially in epidemic diseases, infinitely surpassed [1].

For the illustration of the nature and cure of epidemic maladies, contained in the fifth part, I am to be considered solely responsible. Justice, however, requires me to avow, that the train of thought, and the course of experiment, which have led to the conclusions therein promulgated, were originally suggested by the new and luminous views of the philosophic author of the "Elementa Medicinæ Brunonis;" whom less prejudiced posterity, I venture to predict, will not hesitate to acknowledge as the Hippocrates of the eighteenth century.

To contemplate the uniform perversion of the few great discoveries, which have, at any

[1] I here particularly allude to the deleterious practice of blood-letting. The mass of doctrines imputed to Hippocrates I take as I find them. How far the works issued under that venerable name, are genuine, or adulterated by interpolations, or otherwise vitiated, forms a subject of enquiry, which, as it is not much to my taste, I willingly consign to the more patient industry of schoolmen and commentators.

time, adorned the science of Medicine, one might be almost tempted to believe that an irremediable fatality hangs over the affairs of this most interesting department. Thus, whilst such as are correct of the fundamental principles of the illustrious Brown, are still unapplied, disregarded, or scarcely understood; the grand error, into which he has fallen, in common with all his predecessors and followers, of supposing a class of diseases of excessive excitement,—a morbid state of organs attended with excessive vigor!—has, upon the strength of his additional authority, been the more firmly established. Here we have an ancient prejudice, strengthened by the sanction of a modern doctrine, which the author would, I am persuaded, instructed by farther experience, have had the candour to renounce, made the pretext for continuing a system of depletion, especially in epidemic diseases, which cannot, upon any rational principle, be considered otherwise than highly destructive to mankind. "The frail bodies of men must have an evacuation for their humours, and be phlebotomised."

The labours of Brown appear to have effected more towards approximating medicine to the state of a science, than those of any, or, perhaps, of all his predecessors. But he has left much to be done, even in the correction of his

errors. It is one of radical importance, into which he seems to have fallen from the mere difficulty of soaring wholly above the level of prevailing prejudices, to divide diseases into *three* classes, whilst nature includes them in ONE. In the application of his theory to practice, he had not sufficient experience not to be misleading: and this has been a source of much mischief, as well as of indiscriminate rejection.

In these respects, I have endeavoured to do, what the author would probably have done, had he lived to benefit by farther experience and observation.

As, in pestilence, the dread of infection, and ignorance of the means of cure, have, in Christian communities, served mutually to prolong each other; so, the establishment of correct principles of cure may be expected to dissipate the dread of infection, and the refutation of the doctrine of contagion to facilitate the application of remedies, and to accelerate the improvement of the means of cure.

Of the inferences here published, it may be right to observe, that, but a small portion is exclusively the result of my researches in the Levant: some, as the mode of cure, are the fruit of previous; and others, as the origin of the doctrine of contagion, of subsequent enquiry: The whole may be regarded as the ac-

cumulated product of a long course of experiment and observation ;—

" The crabbed toil of many thoughtful years."

From an early period of life, circumstances had led me to devote much of my attention to the study of epidemic diseases. In 1788, during a voyage to India, I discovered the rudiments of a peculiar method of cure in intermittent fevers. In 1790, this plan of treatment was successfully extended, by analogy, to the yellow fever of the West Indies; at which period an opportunity also occurred of observing, that, contrary to the then almost universally received opinion, that disease does not depend upon contagion; seeing that contact was not followed by the malady, either in myself, or in the attendants. A treatment founded upon the same principles, and similar reasoning respecting their cause, were afterwards successively applied, with increasing accuracy and effect, in the fevers of Bengal, Batavia, and Bencoolen ; and in other acute maladies.

Tracts, sketching some rude outlines of these doctrines, were first published, in India, in the autumn of 1796.

They constituted the earliest attempt to disprove universally the existence of infection, in

epidemic diseases[1]; or to indicate, in any disease, a mode of cure, founded upon demonstrated principles[2].

[1] "*A Dissertation on the source of epidemic and pestilential diseases, shewing that they never arise from contagion, but are always produced by certain states, or certain vicissitudes of the atmosphere.*" As the reader may not have an opportunity of referring to this Tract, I shall here state some of the principal grounds, upon which my refutation of the hypothesis of contagion, in the first instance, rested. 1st. Consequences would necessarily result from the existence of contagion in epidemic and pestilential diseases, which do not actually take place. 2dly. That doctrine has always been taken for granted, not only without proof, but even contrary to the evidence of numerous and convincing facts. 3dly. The air can be distinctly shewn to be the cause of these diseases.—Such are the general grounds upon which I founded my original refutation. The *origin* of the doctrine of contagion, the consideration of which forms a new and important part of this disquisition, I did not trace until after my return from Turkey. The whole, it is presumed, as now presented to the public, will form a demonstration at once complete and satisfactory.

[2] "*A Treatise on the Action of Mercury upon living Bodies, &c. Calcutta,* 1796."

It is to be recollected, that, according to the principles, which I would enjoin, Mercury is prescribed for the cure of diseases, not as an alterative, a deobstruent, a cathartic, a diuretic, a sudorific, a sialagogue, a cholagogue, or a specific; but simply as an exciting power. As such, I first applied it, in yellow fever, in the Island of Jamaica, in 1790; and since that period, in other fevers, and various

In these tracts, it was expressly inferred, that the theories, they contained, respecting the cause of epidemic diseases in general, as well as the mode of treatment, which I had ascertained to be efficient, in other fevers, would be found applicable to the plague[1]. To verify this conclusion, by a practical enquiry into the nature of that formidable malady, consequently became an object of my ambition. For this purpose, I set out for Florence, in the year 1800, in company with Mr. Windham, our then minister to the court of Tuscany, with the view of passing from Italy to the Levant, when the plague should occur in that quarter; that gentleman having promised to aid me with his influence, towards carrying my project into effect. On reaching Vienna, we received intelligence that the troops of France had entered Etruria; which, for that time, frustrated my intention.

acute diseases; and, recently, in the plague of the Levant. The employment of this remedy in yellow fever, in Jamaica, appears to have been the origin, as far as the *name* of the agent is concerned, of that practice, which has since been distinguished by the name of "The Mercurial Treatment," in America and the West Indies; and has, by its misapplication, done so much mischief in all parts of the globe. Of these matters, I shall have occasion to say more presently.

[1] A Treatise on the Action of Mercury upon living Bodies, &c. p. 34.

In the hope of compensating for this disappointment, having just heard that an epidemic had broken out at Cadiz, I applied to the Spanish ambassador at Vienna for permission to repair to that city, in order to treat the malady which was proving so destructive to its inhabitants; relying, from my extensive experience of fevers in other quarters of the globe, upon being able to cure that which prevailed in Spain, whatever might be supposed to be its cause; and offering, as our nations were at war, to remain under *surveillance,* during my residence in that country. The offer was made with the knowledge and consent of a late worthy and lamented nobleman, Lord Minto, our then ambasssador to the court of Austria. The Spanish minister consented; but said he must write for passports to Madrid. Upon which I relinquished the design, predicting to him, what actually happened, that, before he could receive an answer from his court, and I could repair, in consequence of that answer, to Cadiz, the epidemic, following the usual course of the seasons, would cease.

In answer to a similar application to the British government, the same year, having reference to the expedition to Egypt, I was informed that there were no vacancies in the medical department of the army.

Being shortly afterward at Paris, during the peace of Amiens, I entered into a correspondence with M. Chaptal, minister of the interior, upon the same subject. My idea was to induce the governments of Europe conjointly, as I lately endeavoured to do that of Turkey, to institute a plan, for the regular, and continued investigation of epidemic maladies, upon a scale, and in a manner, commensurate with the importance of the object. A memorial, to that effect, which I transmitted to M. Chaptal, being referred, by him, to the French body which corresponds with our College of Physicians *(L' Ecole de Medicine de Paris)*, they reported that my plan was "of too extensive a nature, to admit of being carried into effect."

For some account of these several representations I beg to refer to a small volume of travels, which I caused to be published, in 1804; and which, as it was written without any view to subsequent investigation, (for I had then considered myself as having probably taken leave of the subject for ever) may serve to convey a more satisfactory idea, than any thing I could say, at the present moment, of the uniformity of my purpose[1].

[1] *An Excursion in France, and other Parts of the Continent of Europe, &c. published by Longman and Co. Paternoster Row*, 1814, *p.* 1—25.

The renewal of hostilities between Great Britain and France, in 1803, and the nature of the war, having rendered it improbable that I should, during its continuance, have an opportunity, upon the proper theatre, of prosecuting the necessary enquiries, in person; in order that nothing might be left undone, upon my part, towards promoting so useful an investigation by other means, I placed, in 1804, or early in 1805, in the hands of Dr. Rollo, of the artillery, at Woolwich, a manuscript copy of my dissertation on the source of epidemic and pestilential diseases, with such additions and improvements as had occurred to me since its first publication, in 1796; judging, that, from his situation at the head of the ordnance medical department, he might have frequent opportunities of mentioning the doctrines, which it embraced, to medical men going upon military expeditions, and of recommending their being submitted to trial, if he should deem them of any importance.

Several attempts were, in the mean time, made, by public spirited individuals, apparently upon the grounds which I had suggested, to investigate the plague, in the Levant; but none of these attempts have been attended with success; and the investigators, as is related in my

narrative, have all unfortunately fallen victims of their benevolent endeavours.

After a lapse of years, the increasing frequency of the recurrence of pestilence, in Gibralter, and the provinces of Spain; its unusual devastations, in Malta, and the Ionian Islands; and its almost constant existence, and increasing mortality, throughout the Turkish dominions; again forcibly called my attention to the subject: and, the establishment of a general peace, enabled me, in 1815, although upon a scale very incommensurate with the magnitude of the object, to carry my long meditated project into effect [1].

In now offering to the public the result of my researches in the Levant, concerning the plague, it is on several accounts, gratifying to me, to be able to announce, that I only present to them a practical confirmation, in that disease, of the inductions, which I had previously obtained, respecting epidemic maladies in general. It is doubly worthy of remark, that, whilst respecting the cause of those diseases, the origin of that stupendous error, which I had already refuted from general reasoning, and the nature of things, is now traced in the page of history; the prin-

[1] The Levant Company, by the recommendation of His Royal Highness the Duke of Kent, through their Governor, Lord Grenville, have contributed to defray the expence of this investigation.

ciples of cure, which had, twenty years before, on the banks of the Ganges, been recommended, from theory, to be employed in plague, should, in 1815, upon the banks of the Bosphorus, have been found, in practice, to be suitable and efficient; affording, at once, an illustration, and a triumph of a principle.

The improvements upon the doctrines published in India, which have been derived from my subsequent researches, especially in the Levant, principally consist in a practical knowledge of the seat and symptoms of the plague, obtained by visiting, examining, and handling the patients under my care, six times a day; in additional information concerning the nature and cure, and the cause and prevention, of epidemic diseases; in more copious proofs of the erroneousness of the doctrine of contagion; in the elucidation of the extraordinary origin of that error; in the more full and precise developement of its injurious consequences to society; and in the illustration of the obstacles, both amongst Christians and Mahommedans, which impede, or prevent, the investigation of this class of maladies.

Thus reinforced by the results of a practical investigation of the plague of the Levant, and matured by the additional experience and contemplation of twenty years, it is with unabated

confidence that I now again submit my doctrines, respecting epidemic diseases, to the scrutiny of the public. And, most willing as I should be to benefit by correction, it is with sincerity I assure them, that, if, in some things, I have seen reason to alter, or to modify, my early opinions, upon this subject, it is not, however, in the smallest degree, in reference to those parts, which have been most excepted against, by the critics of my former publications.

The original sketches of a subject so extensive, and so strangely involved, were, as might be expected, extremely imperfect. But, although they form but the mere skeleton of the fabric which I now rear, the evidence of their existence will serve to evince, as the execution of this work will, I trust, farther confirm, that the enquiry concerning epidemic diseases, of which it embraces the results, far from being a hasty, ill-digested speculation, has been an object of the deliberate pursuit of many years: as well as to establish beyond dispute my right of priority, in respect to the conclusions, which I now, for the second time, promulgate, as the genuine and unborrowed results of my peculiar experience and contemplation.

But the presumptuous hope is, by no means, entertained, that my doctrines, thus amended, are not still very far from being perfect. They

cannot, indeed, even yet, be considered more than elementary. The imperfections, however, which depend upon a deficiency of knowledge, respecting the application, in detail, of principles, essentially true, are such as farther experience and observation may be expected gradually to remedy. Distinct landmarks being established, if we occasionally deviate from the right path, we are soon admonished to return: without them, the greater our progress, the farther we proceed from our course.

Having, as I trust, to a certain degree succeeded in erecting such landmarks, for the benefit of future travellers, I may be allowed for a while to suspend, for I do not wish wholly to abandon, my unprofitable, and perhaps unthankful, yet not unpleasant, labours. In as far as depends upon my individual unaided exertions, with the committal of these volumes to the press, the investigation of epidemic diseases may be considered as terminated.

It must, indeed, be obvious, that, an individual, even if he happen to possess far greater adventitious influence, than has fallen to my lot, cannot, in any case, do much more, than merely to announce his discoveries. Their application, and consequently their degree of utility, must depend upon the progress of general conviction. But general conviction, is, in all soils, notoriously a

plant of slow growth: and, in the present case, the obstacles to the application of principles, are of so peculiar a nature, that, it would require the influence of the superior authorities of states, in any essential degree, to counteract them. It is not, however, to propagate even truth, against conviction, that I would invoke the aid of power; but merely to obviate almost invincible difficulties, which impede, or prevent investigation; or, which render the application to use, of results already obtained, impracticable.

If in opposition to very formidable obstacles, as well as in the face of no ordinary dangers, I have been so fortunate as to bring this interesting investigation to an issue, if not complete, at least successful beyond all former example, may I not, without offence, be allowed to express my hope that the executive authorities of states will deem it incumbent upon them, to take some pains, to cause the full benefit of my discoveries to be imparted, throughout their respective dominions, wherever epidemic diseases are liable to prevail? Toward so desirable an object, it can scarcely be necessary to assure them, that my most zealous co-operation, should my experience be thought to have conferred a value upon my services, will be, at all times, at their disposal.

The deductions, of which it is the purpose of this work to demonstrate the correctness, must

be admitted to be important, in proportion as they are extensively applicable. But, as they embrace scarcely any inferences, which were not comprehended, upon a smaller scale, in the tracts upon this subject, which I caused to be published at an early period of life[1], it seems due to my efforts upon that occasion; to science generally; to the public; and to myself individually; that I should here offer some explanations, respecting the following circumstances:

1. The length of time, during which these

[1] The tracts here alluded to were three in number. The first, written in conjunction with my late ingenious, esteemed, and much lamented friend Dr. William Yates, is entitled, "A View of the Science of Life, on the principles established in the Elements of Medicine of the late celebrated John Brown, M. D.; with an attempt to correct some important errors of that work; and cases, in illustration, chiefly selected from the records of their practice, at the General Hospital, at Calcutta." The second is, "A Treatise on the Action of Mercury upon living Bodies, and its application for the cure of diseases of indirect debility;" and the third, "A Dissertation on the Source of epidemic and pestilential Diseases; in which it is attempted to prove, by a numerous induction of facts, that they never arise from contagion, but are always produced by certain states, or certain vicissitudes of the atmosphere." The two latter were exclusively written by me. They were all three published together, at Calcutta, in the autumn of 1796. It is the Dissertation on the source of epidemic diseases principally, which has reference to the subject at present under discussion.

doctrines have remained unacknowledged, almost uncomprehended, and but partially and imperfectly applied;

2. The pernicious effects of their misapplication, as partially adopted, and imperfectly understood;

3. The numerous attempts to appropriate them in detached fragments, prompted by an obscure sense of their superior efficiency, even imperfectly understood, to the results of the ordinary exercise of the art of medicine, in its conjectural state; and,

4. The reasons of my silence, respecting them, during the period, which has intervened, from their first publication.

My doctrines, originally promulgated in India in 1796, were reprinted in America in 1797; imported into England in 1798; and published in Germany in 1800. In the interim, criticisms upon them appeared in the New York Medical Repository, and anathemas in the London Reviews. They have consequently been widely circulated; and of course variously received.

Whilst, upon a retrospective view of the subject, it cannot but be gratifying to me, to find, that, some of the principal changes, which have, in the intervening period, taken place, whether in the theory or practice of medicine, especially in respect to acute diseases, have been entirely

in the sense of the meliorations, which I then suggested, according to the very imperfect manner, in which they have been understood; I can, however, have no wish to conceal that these changes have been slow, partial, and incomplete, in a degree, that, in reference to a subject of science, and to the enlightened times in which we live, does not admit of being easily credited, or readily explained.

In this, as in most, if not in all cases, I fear it must be admitted that the reception and progress of useful innovations, depend no less upon accident and necessity, than upon the force of evidence, and the clearness of perception. Aware how readily doctrines, but little, if at all removed from error or insignificance, if they happen to accord with prevailing interests, prejudices, or passions, of a powerful kind, may be received by acclamation; whilst the adoption of the most important improvements, if they happen to threaten many interests, to disturb many prejudices, or to wound many feelings, may, for a long time, be considerably retarded, or even wholly suspended; I was not permanently either greatly mortified or surprised, at the unfavourable reception, and slow progress, of my discoveries. It seems, indeed, to be the usual course of things, in every department of knowledge, but more especially perhaps in medicine, that doctrines differing

widely from those existing, and established, should be at first misunderstood, misapplied, and rejected; that they should, according to their degree of importance, be successively misrepresented, calumniated, and opposed; and, if true, finally adopted.

For the unapplied state, in which, presuming them to be correct, these doctrines, after having been in the course of open circulation, for upwards of twenty years, still remain, even the acknowledged power of inveterate habit, does not seem fully adequate to account.

Had I, after a sleep of that duration, now first awoke, I should, upon being informed of the time which had elapsed, since the promulgation of my medical inferences, in the East, have expected to find, as they have at least never been deemed trivial, and as it has not yet been pretended that medicine has attained a degree of perfection, which renders it incapable of farther improvement, that they should have been submitted to the test of a rigid but impartial scrutiny; that, if found correct, they should have been generally adopted, and applied; but, if discovered to be partly true, and partly erroneous, that the truths should have been retained, and the errors rejected; or, if ascertained to be wholly destitute of foundation, that they should have been refuted, and rendered incapable of effecting, what experience has shewn that

doctrines destitute of foundation are not always incapable of effecting, considerable mischief.

In order to account for this slow, partial, and imperfect progress (in a science, which concerns the life and health of man, even the smallest progress, if it be in the right course, is of no trivial consequence) we are under the necessity of choosing between one of three conclusions: 1st, That the principles originally promulgated in India, and here re-stated in an improved form, are utterly untrue, worthless, and undeserving of notice, beyond any vague hypothesis that has ever appeared in medicine: or, 2nd, That they have, for upwards of twenty years, remained wholly obscure and unknown: or, 3dly, Seeing that they are not incomprehensible, that their remaining unacknowledged, almost uncomprehended, and but partially and imperfectly applied, is to be imputed principally to the influence of inveterate prejudices and habit.

Respecting the first conclusion, it may be observed, that, if I have succeeded in demonstrating the truth of the principles in question, according to the object of this work, it of course falls to the ground.

The second allegation would be still more palpably untenable. For, that my doctrines have not, during the interval, which has elapsed, since their promulgation, remained unknown,

obscure, is abundantly manifest, as well from the facts, which have been already stated, respecting their early and wide circulation, as from the partial change in opinions, respecting the cause of epidemic diseases, and the more extensive change, respecting the treatment of diseases generally, which have, within that period, been taking place; since it does not appear that these changes can be justly imputed to any anteriour source.

Thus, it was some time after the republication of my "Dissertation on the Source of Epidemic Diseases" at Philadelphia, that Dr. Rush magnanimously proclaimed the renunciation of his former opinion of the yellow fever spreading by contagion: "In the fourth volume[1]," says he, "the reader will find a retraction of the author's former opinion of the yellow fever spreading by contagion. He begs forgiveness of the friends of science and humanity, if the publication of that opinion has had any influence in increasing the misery and mortality attendant upon that disease. Indeed such is the pain he feels, in recollecting that he ever entertained or propagated it, that it will long, and perhaps always deprive him of the pleasure he might otherwise have derived from a review of his attempts to fulfil the public duties of his situation."

[1] Med. Enquiries and Observations, Pref.

Previous to that period, the American Physicians, having, in respect to yellow fever, all taken contagion for granted, were divided into those, who believed, that it was *imported from abroad*, and those, who thought, that it was *generated at home*. Of this latter opinion, at first, was Professor Rush.

Now, if it should appear, that the illustration, which I originally employed, to disprove contagion, as the cause of pestilential diseases, was fitting; that a similar one had not been previously used by others; and, that, no arguments essentially different, or more convincing, were subsequently adduced; it will inevitably follow, that, to the circulation of my opinions and proofs, in America, is due the principal credit of having produced this conviction. Whether it has yet become so general, as to have effected the abolition of quarantine, in that country, I remain uninformed.

Every one, who reflects duly upon the sum of misery and mortality, which Dr. Rush so candidly admits to have arisen from the consequences, even as applied to the epidemic of one country, of that erroneous belief, to which he had contributed to give currency, must shudder in contemplating the tremendous extent, to which these ravages are multiplied, when considered in reference to the epidemics of all the countries, in which this opinion continues to prevail.

Whether we turn our enquiries to the old, or to the new world, we do not perceive, that, in this respect, the signs of improvement are yet more than straggling and incipient. The most vigorous and enlightened minds of either hemisphere, have not hitherto been able to emancipate themselves from the trammels of the grand delusion of 1547, beyond its renunciation, in respect to particular epidemics, as if, instead of being examples, these were no more than exceptions to the general rule. This remark, as it regards the old world, is confirmed by the fact, that, notwithstanding all the additional experience, derived from the histories of the pestilences, which have, of late years, so often afflicted Gibraltar, and the towns of Spain, so partial and insignificant is the change of opinion, which has yet taken place, respecting the cause even of these maladies, that the usual pernicious measures of medical police, depending upon the pre-existing errors, still continue, every where, in full operation.

In all parts of the globe, to which the hypothesis of contagion, as the cause of epidemic diseases, has extended, the inevitable consequence of the inability of self-extrication from that labyrinth of original delusion, is, the necessity, in order to maintain some consistency, of rejecting many true inferences, which are at variance with it, and of forming, in its support, an almost inter-

minable chain of farther impossible conclusions. Thus, whilst a proposition, so clear, simple, comprehensive, and almost self-evident, as, "that no epidemic disease, no fever whatsoever, excepting those, as small-pox and measles, which notoriously depend upon that source, is ever, in any person, upon any occasion, at any time, or in any place, propagated by contagion," is rejected, for no other reason, than that it is inconsistent with a doctrine gratuitously assumed, and now disproved; another proposition, which, upon strict investigation, is found to be utterly erroneous, viz. "that plague, typhus, yellow, jail, ship, and hospital fever, are *sometimes* produced by *contagion*, and at other times by *other causes*," is, apparently for no other reason than the ideal necessity of countenancing the same assumed doctrine, eagerly embraced, as if it were an obvious, or self-evident truth. Thus, when they happen to favour deep-rooted prejudices, conclusions the most false, absurd, and pernicious, are readily adopted upon trust; whilst, when they happen to militate against confirmed habits of thinking, the most correct, palpable, and useful inferences cannot gain admittance to the understanding.

In the duration of the unfounded hypothesis, which regards the cause of epidemic diseases, there is something extremely degrading to the enlightened intellect of the nineteenth century. I know

nothing, to which the consequences of the traditions, respecting contagion, can be more fitly compared, than to those of the fabulous legends, concerning ghosts, which are still employed, in some countries, to frighten children into good behaviour. Both notions are imbibed from original education; and they are both apt to acquire, from habit, the force of an indelible belief. The child, who has been taught, by his nurse, to dread apparitions, connects them, in his mind, with darkness; and invariably expects that they will make their appearance with the return of night. As he advances from childhood, he begins to reason: he finds that darkness does not necessarily bring ghosts: he doubts: he perceives grounds of disbelief: he begins to consider them as imaginary: and terminates by being convinced, that, they have no real existence. But, whilst his opinions are yet fluctuating, although he adheres, by day, to the conclusions, formed by reason, night uniformly brings back a ghost, in every waving bush. In this struggle of reason against early prejudice, victory often remains long undecided: and, in many cases, the ideas implanted in tender age, remain unalterable. I know a military man of tried courage, and now advanced in life, who will not enter a house, after dark, but back foremost, lest legions of ghosts should be closing in his rear.

Just so is it with contagion. In the absence of pestilential diseases, or in the slighter degrees of epidemics, men may be got to reason coolly, respecting their cause. But, when disease, desolation, and death, begin to thicken around, and members of the medical faculty to share in the common calamity, reason becomes silent, and the phantom of contagion, like ghosts in darkness, takes undisputed possession of the unconfirmed mind. This darkness of the understanding will disperse before the torch of discussion, and well conducted enquiry.

But, to return to the progress of my doctrines; from a perusal of the medical writings of the day, in different quarters of the globe, it is impossible not to infer, that, in the treatment of diseases, a change, conformable to them, has been gradually operating, during the period, which has elapsed, since their promulgation. Their adoption, it is true, has, as I have already observed, been slow, partial, and incomplete; the knowledge of the principles of cure, extending, for the most part, very little beyond that of the mere names of the agents, directed to be employed, as remedies. Of mercury, for instance, I then first made known the utility, as an exciting power, in the cure of fevers, and other acute diseases, in which it had either never before been employed, excepting under the delusive

notion of a purgative, or in which it had always been regarded as pernicious. As it now appears, that, whether we look to Europe, Asia, Africa, or America, this agent, without a due regard, however, to the principles according to which all remedies ought to be administered, is employed for the treatment of almost all possible diseases; and as I know of no anteriour source, from whence such a practice could have been derived, I am not aware that I shall be doing an injustice to any person, if I attribute this change in medical practice, such as it is, to my own humble endeavours. Of the destructive consequences to mankind, which have resulted, and do now daily result, from the almost universal misapplication of these principles, but for which I cannot, in justice, be considered in the smallest degree responsible, I shall presently have occasion to speak.

If it should appear, then, that the opinions, which I have promulgated, respecting the nature and cure, and the cause and prevention of diseases, have obtained a wide and general circulation, and that they approach toward correctness; it will inevitably follow, that, they must have hitherto remained unacknowledged, almost uncomprehended, and but partially or imperfectly applied, principally from the influence of inveterate prejudices and habit. If it should be al-

ledged, that, they were not, in the first instance, rendered sufficiently intelligible, I will not affirm that they might not have been much better elucidated. But of this I am well persuaded, that, had they treated of a department of knowledge commencing *de novo, i. e.* in which a particular set of notions (for I cannot call them opinions) were not already accredited, my doctrines, as at first explained, would have been sufficiently intelligible, to have received, in the intervening period, all the farther improvements, of which they are susceptible: whereas, in as far as they are diametrically opposite to the prevailing dogmas in medicine, it is necessary that these should be, in a great measure, renounced, or obliterated, before those can be adopted, or even fully comprehended. Of such renunciation, and of such adoption, both the propriety and importance are here satisfactorily shewn.

If the natural repulsion between inveterate prejudices and truth; if the impossibility of a mind, saturated with error upon any given subject, being open to the impression of correct principles upon the same subject, must be admitted to form of itself, in minds so circumstanced, an insuperable barrier to improvement; it also unfortunately happens, that, there are many collateral impediments, many lesser incompatibilities, which, in society at large, form minor, but

numerous, and powerful, obstacles, to the progress of knowledge, in certain departments. But, as these are different, upon different occasions, each case ought to be considered according to its specific circumstances.

In addition to the general obstacles, arising from those long established prejudices, which operate alike, according to their degree, in all parts of the world, a variety of local impediments, in the immediate birth-place of an important innovation, concur to impede, to repress, or to reject discovery, the notoriety of which has given rise to the common adage, that, "no man is a prophet in his own country;" in this country at least it might have been added, "until he is dead." Repulsion to improvement appears to be in the inverse ratio of distance. Discovery is the less shocking to grovelling self-love, in proportion to the space which it travels over: made by a countryman, friend, or neighbour, it is quite intolerable. Thus, to recur to the immortal discoverer of the doctrine of excitation, whilst his deductions, and his person, were ostracised, at Edinburgh, his theories were received with enthusiasm in the universities of Italy. Whilst, in the latter, they were eulogised even beyond their merits, in the former, every engine of medical authority and misrepresentation, were placed in permanent requisition, to effect their indiscri-

minate and eternal expulsion. Hence, to appreciate their value, at that time, became impossible. And it cannot fail to strike the reader as a matter of curious contemplation, that my remarks upon this subject, twenty-one years ago, should not have become wholly inapplicable even yet: "The strenuous and authoritative manner, in which this doctrine (that of Brown) has always been opposed, renders a knowledge of its application to practice difficult to be obtained, even by those who thoroughly understand its principles. The laws of mechanics may be perfectly well understood. But, if a body of artificers, who had from time immemorial conducted the operative part, in total ignorance of those laws, were unanimously to declare, 'that the principles might indeed be both ingenious and just, for aught they knew, but that they were dangerous in their application to practice,' it is certain that the public would, for a time, be deceived by the representations of these workmen; and the principles of mechanics, however just or applicable, could not generally be reduced to practice, until the deception ceased[1]."

The indiscriminate rejection of such parts as are correct of the doctrines of Brown, on the one hand, and the indiscriminate adoption of such

[1] Postscript to a View of the Science of Life, p. 108.

parts as are erroneous, on the other hand, have alike concurred to retard the progress of medical improvement. " Those who have admired, and those who have opposed the new medical principles, without being masters of the subject, must have been equally unsuccessful, in their attempts to apply them to practice. By every succeeding case of failure, the admiration of the one would be diminished, the opposition of the other confirmed. The objection, therefore, is very just, that ' attempts to apply the principles of the Brunonian doctrine to practice, may be dangerous, in ignorant hands.' In other words, men cannot apply to practice principles, which they do not understand [1]."

Such appeared to me, in 1796, to have been the state of medicine, in respect to the doctrine of excitation, as I shall hereafter call it, in order to avoid circumlocution. At that period, Dr. Yates and myself, taking advantage of the extensive means of practical elucidation, which were within our reach, and which unfortunately had not been accessible to the original promulgator of that doctrine, endeavoured, in the tracts, which I have already mentioned, to give an amended view of the subject, distinguishing what appeared to approach correctness, or to be ca-

[1] Postscript to a View of the Science of Life, p. 9.

pable of farther illustration and improvement, from what was considered as wholly erroneous, and proper to be rejected, of the doctrines of the "Elementa Medicinæ Brunonis."

These amendments, as has been already stated, have, in some measure, hitherto shared the fate of the original work. They have been alike discredited by the conduct of their supporters, and opponents. That principles, but imperfectly understood, should be uniformly misapplied; that their miapplication should produce mischief, and mischief objection; as well as, that, the evils, thus occasioned, should be imputed, by the persons producing them, as faults, to the doctrines, which they have misunderstood, rather than to any want of apprehension, in themselves, are all in the usual relation of cause and effect.

I shall here briefly advert to a few of the most prominent instances of the misapplication, to which I allude.

In the repetition of the doses of remedies, for the cure of diseases, we have a striking example of the dread which is entertained, of deviating so far from the usual routine, or prejudices of medicine, as to wander into the correct application of a principle. It is one of the most fundamental, and most extensively useful principles of medical science, that the doses of remedies should be administered at intervals equal to the duration

of the action of each dose. The exact observance of this rule is indispensable, first, to the cure of the original disease; and, next, to the prevention of new diseases, which would otherwise arise from the diminution, suspension, or cessation of the action of the remedies. If two hours be the estimated period of the duration of the action of each dose of any agent, it ought also to be that of the interval between the successive doses. Notwithstanding the length of time that these principles have been divulged, there are yet very few persons who will venture out of the beaten tract of administering remedies, into a mode that is founded upon the exactness of correct deduction. Some will give a remedy once, twice, or, at the utmost, three times in the day, that ought to be administered every two hours, or every hour, according to the duration of the action of each dose. Some, who are bolder, may be induced to repeat them every four, or every three hours: but, the number of those, who, by repeating them at intervals equal to the duration of the action of each dose, will venture to embrace a perfect principle, is extremely small. In approaching the point of correctness, they seem as if they were approaching a precipice.

Practitioners, I have had occasion to observe, have also sometimes committed an error on the opposite extreme, in the repetition of the doses at

intervals shorter than the period of the duration of the action of each dose. And this, although not of so dangerous a tendency, is, as a deviation from principle, also to be avoided.

All the exciting powers, commonly called remedies, are known to be capable, by their misapplication, of producing diseases as severe, as those, which, by their due application, they are capable of curing.

The original malady being removed, the effect of not continuing to repeat the doses of the remedy, which had been employed for the cure, if it be one of high-exciting power, at intervals equal to the duration of the action of each dose, or of not substituting some other exciting power equivalent in force, thus gradually reducing the sum of excitation, is the production of a disease equal to, or greater than, that, which had been removed. The manner in which diseases thus arise from the misapplication of remedies, appears to have been but very imperfectly, if at all, comprehended.

Since, in consequence of the diffused knowledge of the doctrine of excitation, but imperfectly understood, the employment of exciting powers of high degree has become general for the treatment of severe maladies, the production of disease, in consequence of the misapplication of remedies, has also prodigiously extended.

Of all the exciting powers, which are, or have been, employed as remedies, this observation applies more especially to mercury; which is now administered in almost every species of disease, and in all cases of emergency, in which the practitioner is at a loss for precedent. This agent has, in consequence, become one of the most frequent, and most pernicious, causes of sickness.. The destruction that has been occasioned, by its constant misapplication, for three centuries, in the treatment of lues venerea, has been truly enormous. But, of late years, the evil has been augmented in the precise proportion in which it has been applied, as a remedy, in a greater variety of diseases: insomuch, that it may now be almost a subject of doubt, whether this agent, or the air, has the greatest influence, in occasioning, or aggravating, dangerous maladies, throughout the world; as consumptions, in Great Britain.

So universal, indeed, has the use, and the abuse, of this agent, become, that complaints have been made, with much justice, by some of the physicians of the metropolis of this nation, that it pervades even the nurseries[1]. But they seem not to have been aware, or not to have recollected, that the evil is not to be removed, by declamation, or

[1] Observations on the hepatitis of India, and on the prevalent use of mercury, in the diseases of this country, by W. Saunders, M.D. &c. p. 20.

the proscription of the remedy; but by explaining the manner, in which salutary, and noxious effects, *i. e.* health and disease, succeed its operation. Not to speak of absolute mortality, the dilaceration of gums, defedation of breath, destruction of complexion, and all the various evils, which are perpetually arising from this source, constitute, in the aggregate, so vast a sum of calamity, that I cannot hold myself exempt from the obligation of giving to this subject, at some early opportunity, the separate consideration, which it appears so richly to merit.

For the augmentation of evils, which has thus incidentally arisen from the misapprehension, and misapplication, of doctrines, which ought to have been better understood, I cannot consider myself, in any degree, responsible. It has, in no manner, depended upon me, that principles, certainly not unintelligible, should have remained so long but imperfectly, if at all, comprehended by the public. Neither is it my province to explain, whether they have been misapplied, or remained unapplied, by the medical faculty, from deficiency of comprehension, ignorance of their existence, professional prejudice, factious rejection, servile support of opposite, and irreconcileable modes of practice, or from combinations of some or all of these circumstances. In shewing, that, when correctly applied, they are efficient for their object, I shall

have performed my part of the duty. It will remain for communities to take such measures, in consequence, that they may no longer continue to be the dupes of the constant misapplication of remedies, and the victims of the constant production of disease.

The diseases, occasioned by the misapplication of remedies, like all other morbid affections, consist of a diminished excitement of some organ, or organs, or parts of an organ, upon which the agents, whose exciting powers have been diminished, or withdrawn, had preferably operated. These affections are indicated by a certain set of phænomena, usually called symptoms. Amongst the symptoms, indicating the diminished excitement produced by the misapplication of mercury, which are very numerous, may be ranked, purging, ptyalism, hæmorrhage, sweating, fœtid breath, swelling and ulceration of the gums, fauces, and tongue, &c. &c. &c. These phænomena, then, instead of being, as has been preposterously considered of some of them, efforts of nature to carry off morbific matter, as if disease were a substance, and not a condition of the organs, are merely signs, or indications of maladies, of various degrees, occasioned by the sudden diminution, suspension, or cessation of the action of the agent, after having been previously applied in force as a remedy.

In the same manner, every evacuant operation, whether by emetics, cathartics, sudorifics, diuretics, cholagogues, sialagogues, &c. &c. &c., is but a sign of diminished excitement, or disease, of the organ which produces it: and the error of considering them, as either causes of disease, or means of cure, is of infinite detriment to the progress of medical science, as well as to the lives of men.

All these, and many other, important positions, are illustrated in the chapters, which treat of the nature and cure of epidemic maladies. From whence results, an elucidation of the grounds of much of the existing confusion, concerning the operation of remedies; and especially of the disputes, whether mercury be capable of curing pestilential diseases, by virtue of its exciting power, or of its action, as a cathartic, diuretic, sudorific, sialagogue, cholagogue, &c., or whether it be capable of curing them at all.

I must here with reluctance remark upon certain representations, concerning the treatment of epidemic diseases, which have the appearance of disingenuousness, although they may only be the result of a confusion of ideas necessarily attached to the imperfect state of medical knowledge. I allude to reports of successful results, from the heterogeneous combination of blood-letting with mercury, than which no two modes of treatment

could possibly be more opposite, or irreconcileable. It has been usual with persons, making these combinations, when recovery has taken place, to attribute the happy issue to the loss of the blood only. But, if they had been convinced of the correctness of their own inductions, it should seem a natural consequence, that confiding in the efficacy of that evacuation solely, they would have abstained from impeding the salutary effects of the blood-letting, by the admixture of so counteracting a mode of treatment, as the administration of mercury must necessarily be. That, whatever is fitting in any one case of disease, must be equally fitting, in its degree, in every other case of the same disease, may be considered an undoubted axiom in medicine. Consequently, if blood-letting were capable of effecting a cure, in any one case of fever, it would be equally capable of effecting cures, in all cases of the same fever, which are not incurable. But not one, even of its advocates, will venture to assert that this evacuation is uniformly good, in all cases of any particular disease: and it will not be denied to be a directly debilitating operation. If, therefore, it were possible for any person, who has had adequate experience of the efficacy of mercury, applied according to proper principles, for the cure of diseases, to conceive it an eligible mode of proceeding, to retard the progress of its

salutary operation, by blending it with blood-letting, in cases, which might safely admit of a slow process of cure, it would not surely be in respect to pestilential maladies, in which the progress to dissolution is rapid, and which are of the very essence of debility.

To this practice, which I shall for the present call only inconsistent, some writers have erroneously attributed the smallness of the number of deaths, which happened at Gibraltar, in consequence of the epidemics of 1813, and 1814. I certainly shall not undertake to prove the contrary, any more than to demonstrate, that the cessation of the epidemics in the towns of Spain, at the usual periods of termination, was not the result of the energetic measures of the medical police, as has been very gravely asserted by others. In the mean time, however, I beg to warn all those, who have an interest in the welfare of that garrison, against giving the smallest weight to these delusive and pernicious notions: and to assure them most solemnly of my conviction, derived as I believe not from a superficial view of the subject, that, whenever the pestilential properties of the atmosphere shall again occur in the same noxious degree, as in 1804, neither the utmost vigilance of the medical police, nor the full benefits of blood-letting,

purging, and salivation, together with every other mode of evacuating the humours, will be adequate to prevent the recurrence of an equal mortality.

That the smallness of the mortality, in these years, ought to be attributed to the mildness of the pestilential influence, will be obvious, when we consider, what is elsewhere fully explained, that the mode of treatment pursued, consisting of a heterogeneous admixture of blood-letting, purging, sweating, and the administration of mercury, without any apparent rule, or principle, or any perceptible object beyond mere evacuation, could not be useful but by chance; and that blood-letting, which appears to have been principally extolled, is so far from being, in any case, a proper mode of treatment, that, although men in health, or slightly affected with disease, *may survive great depletions*, and, from a confusion of ideas, not uncommon in medical reasoning, be represented as recovering *in consequence of them;* it is, in every case, directly and powerfully mischievous: and, 2dly, that, mercury, and other exciting powers, although, when not given in sufficient quantity, useless, or, administered in an improper manner, injurious, will, when applied, in a due degree, and at proper intervals, be found capable of effecting a certain,

speedy, and efficient, cure, in every case of epidemic disease, in which extensive organic lesion has not already happened.

Under this view of the subject, what have been represented as cures from blood-letting, will be regarded as, in reality, cases, in which the patients have been *fortunate enough to survive great depletions;* and what have been represented as failures of mercury, as, in truth, *failures of the practitioner in its application.*

The history of medicine, more than that of any other branch of human research, evinces how utterly unsafe it is, in deducing scientific conclusions, to place any reliance upon that kind of proof, which is of the nature of judicial evidence. In order to be fully sensible of the truth and importance of this observation, we have only to reflect, that, if this species of evidence were good for any thing, we should have more than enough of it, to prove, ten times over, that, plague, and other epidemic diseases, depend upon contagion; that phlebotomy is the proper method of cure; and, that mercury, in whatever manner administered, is useless, or injurious, in its operation: whilst we find the reverse of every one of these propositions capable of being demonstrated, by a series of logical inferences, as well as by the reproduction, by similar processes, under similar circumstances,

of the alledged results,—the only species of evidence admissible at the bar of science.

According to this mode of probation, the cure of epidemic diseases, is shewn to consist in the application of a degree of excitation, proportioned to the diminished excitement, which constitutes the malady, and of a kind determined by the nature of the organs, in each case, principally affected. In proportion as these affections are complicated, or various, the means of cure are complicated, or various, also: and, as those organs, upon which mercury preferably exercises its exciting power, are often chiefly diseased, it follows, that, in performing the cure, this agent, either conjointly, or separately, is a remedy of very frequent application; although not, by any means, considered as either a specific, or a panacea.

I cannot be unaware, that, against the principles of the doctrine of excitation, as here modified and explained, reproaches continue to be *privately* but assiduously urged, that they involve the application of excessive and pernicious quantities of remedial power; that they offer nothing but desperate cures for desperate diseases. No reproach could be more unfounded, or unprincipled. Misrepresentations of this description, however, as they elude refutation and exposure, may, for a while, have the effect of

withholding from the public the benefits of the application of correct principles, toward the preservation of their health and lives; as well as from their advocates their just share of the ordinary emoluments of the profession. To these inevitable consequences, he, who would effect improvements of importance, in medicine, will submit without repining: but he will not submit in silence.

A refutation of the doctrine of excitation, either public or private, I have not yet seen, and I do not expect ever to see. The reproaches clandestinely circulated, powerless but from their incapability of being met, had they been openly urged, and in a manner deserving of notice, might have been triumphantly repelled. The very essence of just principles, in respect to the cure, it may be observed, consists in adapting the sum of remedial power, to the sum of existing disease; in administering high degrees of excitation in high degrees of disease, and low degrees of excitation in low degrees of disease. A charge against doctrines maintaining those principles, so obviously unfounded, as the employment of desperate, or inappropriate, or excessive means, of attaining definite ends, could therefore only have been made, in utter ignorance of their nature, or in extenuation of the notorious inefficiency of the common prac-

tice of the conjectural art of medicine, and to apologise for the flagrant absurdity of continuing to employ inadequate means of cure: for the essence of that practice, is the indiscriminate application of low degrees of exciting power, in high, and low, degrees of disease; *i. e.* to place all degrees of disease upon a level.

This is not an overcharged description of the method of treatment still generally in use, amongst the cultivators of medicine, in so far as the principles of the doctrine of excitation are not adopted and applied, whilst they are anxiously disavowed: and what I have said of these conjecturalists, upwards of twenty years ago, has not yet ceased to be applicable to them: "Let us suppose a person, wholly unacquainted with the *laws* of living bodies, applying *powers* to them; how can he be expected to produce a *given effect?* Overlooking the immense variety of degrees, between the state of health, and the highest state of exhaustion, he would probably prescribe one grain of a solid medicine, when he should have prescribed twenty, or twenty when he should have prescribed but one; he would give twenty drops of a fluid, when he should have given two hundred; or two hundred, when he should have given but twenty. He would repeat the medicine but once, or twice, in the twenty-four hours, instead of every hour, or

half hour, according to the duration of its action. He would use the strongest powers, instead of the weakest; and the weakest, instead of the strongest. He would not make any distinction between the delicate female, and the robust male frame; between childhood and youth, and youth and old age; between recent, and long standing diseases. He would not even know how to make allowances for inveterate habits. In such hands, no success could be expected, any more than from a mechanic, who should employ equal powers to raise unequal weights. He might sometimes, indeed, be right by chance.

"Far otherwise is it with him, who applies principles to practice. He calculates, combines, and proportions his powers, according to known laws; and applies them, in such a manner, as to produce certain and given effects [1]."

Perhaps those, who cavil against the doctrine of excitation, without understanding it, will be surprised to learn, that, so far from having any danger to apprehend from the application of high degrees of exciting power, in high degrees of disease, similar degrees of power may also be employed, both with safety and advantage (although they are not necessary), in low degrees of disease, when the rules, which ought to regu-

[1] Postscript to a View of the Science of Life, p. 109—11.

late the administration, but especially the means of obviating the effects of the sudden cessation of the action, or sudden diminished excitation, of the remedies, are fully understood, and practised.

But, whilst, in most places, the progress of correct principles, has been retarded, partly by ignorance, or a defective knowledge, of their nature; in some situations, a similar effect has been produced, by the very organization of the different branches of the medical profession, in their relation to each other, and to the community; as when it has happened, that, the interests of their members has been placed in direct opposition to their duties.

From such motives, doctrines, that, by simplifying a profession, threaten to diminish the profits of a trade, will always meet with the most decided hostility; if not by being openly controverted, as when such a mode of opposition would only tend to accelerate their progress, at least by the not more incongenial, or worse understood, although more disreputable means of secret calumny and misrepresentation. This observation applies to the affairs of medicine, if not exclusively, at least more strongly, in this country, than in any other. It is, I believe, the only civilised nation, in the world, and there is, perhaps, no uncivilised nation, in which men are

not only permitted, but authorised by a judicial decision, to prescribe and to dispense the medicines, upon the *quantity* of which, administered to their patients, depend *the profits of their trade*[1]!

This decision, the legality of which, since it appears to have had the sanction of only one branch of the legislature, it may be permitted to question, arose, upwards of a century ago, under the following circumstances. An apothecary, named William Rose, was, in 1704, convicted of prescribing; and judgment was obtained against him, by the College of Physicians, in the Court of Queen's Bench, over which Mr. Justice *Holt* then presided. This cause having been afterwards brought, by a writ of error, into the House of Lords, that body, upon grounds, of which I am unable to perceive the justice, policy, or expediency, thought fit to reverse the decision of the court below: and the apothecaries have, ever since, appeared in the sanctioned character of pharmaceutic prescribers; the physicians as their occasional

[1] A Frenchman, German, or Italian, would laugh at the idea of being prescribed for by a pharmacien, or apothecary. And if the physician's fee in foreign countries, is regulated, with respect to the poor, he is remunerated by the rich, in the same manner, as if no such regulation existed.

underlings, or drudges; and the patients as the constant victims of this reversal of the usual order of nature.

From this state of things, about this period, first arose the dispensaries, instituted by the physicians, for the following, amongst other reasons, as stated by one of themselves: "Because the physician is not obliged to prostitute his honour and conscience, by *overloading his patient*, (a frank acknowledgment that they had been under the necessity of doing so) *to oblige a craving apothecary;* or to run the risk of *being undermined in his reputation by slanderous suggestions, for not submitting to be the apothecary's under-pickpocket* [1]."

During the century, which has since elapsed, the dependence of the physicians, and of the public, upon the apothecaries, instead of being lessened, or removed, has, in this country, been rivetted and augmented: and the actual mal-organization of the different branches of the medical profession, as it affects the health and lives of the community, is, undoubtedly arrived at that extreme degree, which calls for the early and attentive consideration of the legislature. But with this mal-organization, I have here no

[1] An Answer to the printed Case of William Rose, &c. London, 1704. Med. Obs. Vol. iv. p. 368.

farther concern, than as it may be presumed to have affected, in this country, the progress of medical improvement in general, and the adoption of the doctrine of excitation in particular.

To this end, I may observe, that if it be the effect of the principles enjoined by that doctrine, as it is the professed object of all science, to simplify practice; if the practice of medicine, in this country, be almost exclusively in the hands of the apothecaries; if the profits of their trade depend upon quantity of medicine, and complexity of prescription; and if they hold the body of physicians, as well as the public, in a state of dependency; it inevitably follows, not only, that, it is strongly their interest, but that it is very considerably in their power, to retard the progress of doctrines, which would simplify, or render efficient, the practice of medicine, as well as to frustrate the endeavours of the votaries of medical improvement. To deduce the consequences farther, would appear to be superfluous.

It is with regret, that I feel myself called upon, to animadvert upon some circumstances, connected with the previous fate of the doctrines, re-inculcated in this work, which may be considered, to a certain degree, personal. After the first re-action, created by new opinions, has, in some measure, subsided, and their truth and

importance have begun to be felt and acknowledged, it usually happens, that attempts are made, by various persons, to appropriate the credit due to priority in their promulgation : and it is not without precedent, that claimants should appear, from amongst those, who had, in the outset, contributed the most to misrepresent, or to defame them.

Such has been the case here. Respecting the principles in question, if there were not other, and better proofs of their validity, a favourable augury might be deduced, from the eagerness, with which minute limbs, or fragments, in a disfigured state, mere counterfeits of them, have, at various periods, been sent forth, as original productions, by medical candidates, for practice, or for fame, in different quarters of the globe; suppressing, as in other cases of furtive appropriation, the acknowledgments, which liberality, politeness, candour, decorum, and honesty, alike enjoin.

That I may not appear wanting in a due sense of the importance of the subject, or in a becoming attention to the maintenance of my personal rights, as well as to obviate the suspicion of plagiarism, when I only happen to quote opinions, after having passed through successive hands, which, however deteriorated, or disguised, were, in their original form, part of

my own inductions, it seems necessary that I should advert to these futile but impudent attempts, at appropriation.

From the existence of my doctrines, in an open course of circulation, for upwards of twenty years, it was competent to many persons, in many places, particularly as my inferences admitted of so much subdivision, to pass locally and partially for their inventors. Thus, whilst some were endeavouring to obtain a credit, which they could not but have known did not belong to them; others, it may be admitted, might have imagined such deductions, forgetting where they had originally acquired their knowledge, to have been processes of their own understandings: but it can scarcely be deemed to be within the verge of possibility, at least we know of no example of such a coincidence, and the proof of its existence would require to be established upon the most unquestionable foundation, that two, or more, persons, might have simultaneously prosecuted a similar train of inferences, and arrived at a similar chain of conclusions, throughout a long series of investigation.

In such matters, the only admissible evidence of right, is priority of promulgation. If thousands should subsequently lay claim to the credit of having been the first to ascertain similar re-

sults, announcing them to have lain for consideration a quarter of a century in their closets, the person, who first gives publicity to new conclusions, can alone have a right to the title of their discoverer. The nature of this right, is, indeed, to me, so clear, that, had I, after having arrived at any important conclusions, been so remiss, or indifferent, as to have allowed them to remain upon my shelf, until after the promulgation, by other persons, of similar inferences, I should have felt it more fitting to have committed them to oblivion, or the flames, than to have insulted the world, by urging the ridiculous plea of being anticipated, which, if it had even any foundation in truth, no one would, or ought to believe. The improbability, that the inordinate vanity, which could prefer such false, or at best doubtful claims, should, for a moment, allow any thing in the shape of a discovery to remain unpromulgated, will, on this point, sufficiently justify incredulity.

However they may have flourished for a moment, it is seldom, I believe, that unfounded pretensions of this nature have been known permanently to maintain their credit. If their authors, by contriving to identify their own proceedings, in the eyes of the public, with those of real discoverers, to pass their base alloy for pure gold, have occasionally succeeded in acquiring

an adventitious celebrity; the novelty of their pretensions having ceased to amuse, and the efforts of the factions, which hostility to those whose fame they would usurp, rather than their own merits, had raised in their favour, having subsided, these counterfeits, the cause of their adventitious and momentary elevation, being removed, have naturally re-descended to the standard of their intrinsic value. Vesputius could not appropriate the fame of Columbus; nor Darwin that of Brown.

If this were not the case, what would have become of investigation? What motives could men, feeling a portion of the inspiration of genius, have for devoting their time to the research after new facts in science, if at the moment of their promulgation, or at least of the acknowledgment of their utility, the credit of their discoveries might, upon such a plea, be claimed, by some intellectual pauper, at the distance of thousands of miles from the scene of their enquiries? Is not the produce of every man's labour his property? Are not experiments, researches, and intellectual processes, of every kind, attended with labour? Does not the credit of a discovery in science as much belong to him, who makes and first promulgates it, as the produce of his fields to the farmer, who sows, reaps, and brings it to market? The

credit due to the intellectual labourer, for having, by new, dangerous, or tedious processes, arrived at new and important conclusions, is undoubtedly of the first-rate order: and it cannot certainly be less criminal to rob him of this credit,—too often the sole reward of the most arduous and unceasing industry, than the cultivator of the earth, of the fruit of his corporeal toil.

But, on the side of the former, there is this farther essential disadvantage, that the thefts, or robberies, by which he suffers, are not equally capable of being reclaimed, or avenged, by any of the usual forms of legal proceeding. He may even be placed in the painful predicament, of being obliged, by tamely abandoning their different ramifications a prey to separate pretenders, to suffer himself to be plundered of the credit of all his discoveries in detail, or, by entering upon just reclamations, of incurring the imputation of being vain glorious. But, if he must choose between either, there can be no question, with a man of spirit, which he ought to prefer.

It is far from being my intention; indeed it would be an endless, and I persuade myself an uncessary task, to specify individual instances of the exercise, in respect to these doctrines, of the species of injustice, which I here proclaim, since the charge of having adopted, without acknowledgment, limbs, fragments, or single propositions, of

them, and passing them as their own, whilst they have neglected to cultivate or improve the principles as a whole, and thus to advance the progress of medical science, appears to be common to all the persons, in this country at least, who have, since their first promulgation, treated the subject of epidemic diseases.

Besides forming in their abandoned state, the unacknowledged foundation of volumes, and of Lectures, of papers, and of Essays, as well as the subject of innumerable and daily minor plagiarisms, in various quarters of the globe, the facts are as true, as they are curious, that some of the principal results of my experiments and researches, as published, in India, in 1796, have since been transmitted, by others, as original inferences of their own, from Edinburgh to Calcutta, from London to Constantinople, and recently from Germany (addressed to myself, whilst in the pest hospital near the Seven Towers, in the act of verifying my doctrines, in the plague) to the Levant [1].

[1] This circumstance, which relates to a communication from a respectable physician of Hungary, who addressed me with the most benevolent intentions, and without any desire, I am persuaded, of passing my opinions as his own, affords satisfactory evidence of the silent progress of my doctrines in that quarter. If, with regard to the other cases alluded to, I abstain from mentioning names, or stating par-

To the ingenuous mind, no subject can be either more difficult, or more painful, than that, upon which it is compelled to take some note of its own comparative exertions. That this disagreeable and embarrassing task has been forced upon me, by the necessity of doing myself justice, against the encreasing encroachments, upon my right of discovery, since the utility of my doctrines has become more apparent, will readily be perceived, and admitted, by all candid persons, who attend both to the fact, and to the reasons of my silence, upon the subject, for upwards of twenty years. And they will feel that some explanation has now become the more indispensable, upon my part, since within a very few days, a critic has permitted himself not only to deny the validity of my claims to my own discoveries, but

ticulars, I beg it to be understood, that, it is neither from any want of materials, nor from an opinion that a narrative of the transactions in question would not be amusing, if not interesting, to the reader; but because it would occupy too much time, as well as too much space, and is not necessary to my purpose: and because I have no wish to enter gratuitously upon an inculpation of individual members of the medical profession. I shall therefore here merely observe, that the Edinburgh discoverer, in a letter to a friend in India, if he did not employ my words, quoted at least the sense of my expressions, in announcing the probability that mercury would be found a cure even in the plague.

even to class, and to confound, my opinions with those of some of the detail pilferers of my ideas.

It would be superfluous to enter, in this place, into a more particular statement. The body of the work exhibits the precise nature of the doctrines, of which I desire to be exclusively considered the author: and when either their correctness, or my claims to originality, shall be attacked, in a manner that may seem to deserve, or to require, refutation, I shall be ready to defend them. In the mean time, I may be allowed to observe, that, to prefer claims, of which the falsehood, and injustice, admit of being so readily detected and exposed, argues, besides supreme turpitude, unspeakable folly. The decision, concerning the merits of such pretensions, rests upon the most simple, and obvious, grounds. There can be only one correct doctrine upon one subject. All those, which are similar, but of more recent appearance, if it were even possible, that, by some surprising coincidence, they should also be original, must, in fairness, be deemed to be only copies: for, it would be of the essence of absurdity, to suppose, that similar discoveries, if even of an antecedent date, should, whilst only laying upon the shelf, constitute the foundation of those, which had been previously promulgated, or that they ought to take precedence of them.

To be indifferent to the issue of such attempts,

would, on my part, I trust, be to under-rate the importance of my inferences. To affect to be indifferent to them would be both foppery, and hypocrisy. No man, who is capable of making a discovery, can be indifferent to the credit of a discovery, that is worth the making. In fact, none of importance has ever been made, of which the credit, upon the prospect of its final establishment, has not been claimed by several persons; and in whose disputes respecting it the public have not taken almost as warm an interest as the respective claimants. But, aware that it would be a transgression upon propriety, as well as upon the patience of the reader, to mix, more than is absolutely necessary, to the maintenance of my private rights, reclamations, which are in any degree personal, with discussions strictly of science; I shall not here go beyond what seems indispensable for the attainment of that object.

There is one publication, however, which it is necessary that I should specifically notice, because in it appears to be embarked all the logic, rhetoric, and reputation, of those, who, however they may think it expedient to avoid a direct or open collision, are, in spirit, the most determined opponents of some of the leading principles maintained in this work; and because it notoriously involves some of those acts of injustice, to which I have been alluding. The author appears to take great

credit to himself, for having, without a single new illustration, or even any improvement of the old, shewn, fifteen years afterwards, what I had already proved, in 1796,—that yellow fever does not depend upon contagion [1]. He farther seems to exult, in having, as he thinks, set the question at rest, concerning the contagious nature of Plague, and Typhus. In these respects, the reader, by the time he has perused this volume, will be completely able to judge how far he has been a true prophet. But, what is of still greater importance to the public, in as far as the writings of the persons in question may have any weight, neither this author, nor any other upon the same subject, have understood, or they have omitted to apply, the efficient doctrines, respecting the cure of epidemic diseases, which were, at the same period, not unintelligibly as I think, and are now still more clearly, set forth. In self-justification, it appears to have become necessary for these writers to deny that they have ever seen my former tracts, containing the doctrines now republished; to refute them; or to acknowledge that they have done me an injustice, and neglected their duty to the public; or, finally, by their silence to acquiesce in the construction which we may choose to put upon their conduct.

[1] An Essay on the Disease called Yellow Fever, by E. N. Bancroft, M. D. &c. Advertisement.

These persons are placed in a truly awkward predicament. By successfully maintaining the first position, they would stand self-convicted of inexcusable negligence, in not having made themselves acquainted with professional opinions, which have not been deemed trivial, and which have been in an open course of circulation for upwards of twenty years [1]. Should they prefer the second position, this work is directly at issue with them. But, should they, on the other hand, make choice of the third option, avowedly acknowledging themselves guilty of individual injustice, and dereliction of public duty, it would be an act of courage and of retribution, much beyond the energy and the candour, which we ought to expect from ordinary minds. Under some circumstances, as when the measure pursued must be odious, but the odium can be divided, the least unsafe course is perhaps silence. But this appears to be one of those unlucky cases, in which silence would be the most expressive mode of acknowledging guilt.

[1] The persons, who have written upon epidemic diseases, in this country, being in general assiduous collators of the opinions of others, have no doubt had frequent occasion to peruse that respectable work, the New York Medical Repository, and to consult other periodical publications, as well as medical circulating libraries; in some of which, if they were not exceedingly unfortunate, they could hardly fail to have met with some traces of the opinions in question.

Under the proper head, the culpable rashness is shewn, of those inexperienced medical pilots, who, in attempting, in particular epidemics, to shun the Scylla of contagion, have fallen into the Charybdis of Marsh Miasma. Such is the strange reluctance of these writers, to admit the power of the atmosphere, derived from its general properties, to any participation in the production of epidemic diseases, that they suppose them, in every situation, as well where it does, as where it does not exist, to depend upon Marsh Miasma [1].

What is most remarkable of the volume here under consideration, is, that, independent of the repetition of the least apt part of my reasoning against contagion in yellow fever, on the one hand, and of the jejune traditions of Italy and the Levant, in favour of contagion, in plague, on the other, it should contain no new facts or observations, of the smallest utility, respecting the cure of epidemic diseases; no specific elucidation, concerning the means of prevention; nor any remarks of value, even in corroboration of what had previously been stated, or hinted, by other persons, respecting quarantine, lazarettos, and

[1] Bancroft's Essay, passim. Dr. Burnet, although he has not, like Dr. Bancroft, erected the opinion into a pernicious system, has also fallen casually into the error. *A Practical Account of the Mediterranean Fever. Pref. p. viii. and p.* 276.

plague police establishments. Instead of a disquisition of science, the reader might fancy himself perusing a collection of light extracts from books of voyages and travels, merely calculated for his amusement; until he perceived that the object of the whole, was to prove, upon such very adequate authority, that there is scarcely any difference, in respect to health, and disease, between living in a pure, or in an impure atmosphere; that the huts of the Greenlanders and Esquimaux, or the cabins of the inhabitants of Kamstchatka, are little less salubrious than the palaces of Great Britain, France, and Italy; that no danger of disease can arise from animal substances in a state of putrefaction; that the air seldom or never produces fever; and that this disease cannot be generated but by Marsh Miasma, even upon a rock [1].

The mischiefs of such doctrines are, indeed, likely to be sufficiently counteracted by their very

[1] Essay, p. 87—156. The inferences, which, in fair reasoning, follow from the doctrines maintained by Dr. Bancroft, go probably much beyond what he intended, and certainly much beyond what is correct. Some of them are more particularly examined and refuted, under the heads to which they respectively belong. It is to be regretted that medical doctrines should still be compelled to look for their support to such uncertain authority as that of voyages and travels; and that men of Dr. Bancroft's talents should not endeavour to emerge more completely from the trammels of scholastic discipline.

absurdity: and, in serious refutation of them, I hold it unnecessary to adduce any evidence, beyond the brief statement of a single fact. A typhus of the most unequivocal description, may, at any time, be produced, on board a ship, at the distance of a hundred leagues from any supposed source of contagion, and from Marsh Miasma, by the simple operation of shutting down the hatches, and keeping the persons on board confined between decks, for a very few days [1].

[1] "Towards the latter end of January, 1811, two English transports (Metcalf and Phyllerea) arrived in the bay of Cadiz from Gibraltar, having between 4 and 500 German recruits on board." They "had been kept on board under quarantine for upwards of a month in Gibraltar bay; and unfortunately on the arrival of the transports in Cadiz, the weather became so tempestuous, that the crews of those vessels, and the soldiers, were obliged to be kept below." "During *the few days* that the hatches were covered over in consequence of the heavy rains, a complete typhus fever had been formed." *Sir James Fellows's Reports, p.* 240. Here there was no alledged source of contagion at hand—there was no fever at Gibraltar, or Cadiz.—It was not even the epidemic season.—The people were healthy whilst they could keep the deck—but after being *a few days* under the hatches, they were seized with a fever.

Sir John Pringle says, "I have observed the same sort of fever (hospital or jail fever) take its rise in crowded barracks, and in transport ships, when filled beyond a due number, and detained long by contrary winds, or *when the men were kept at sea under close hatches* in stormy weather." *Obs. on the*

Whether the members of the faculty, upon whom it was more especially incumbent to make themselves acquainted with, and to turn to the greatest advantage, all that had been written concerning epidemic diseases, are most to blame for their sins of commission, or of omission, in reference to the doctrines here re-published, I leave it to others to determine. On the part of Dr. Bancroft, it would have been but candid, in attempting to refute the medical heresy, that plague and typhus do not depend upon contagion, to have distinctly stated the nature and source of the opinions, which he was opposing: and if it had been his intention to meet the question fairly, we may presume that he would have done so. If there were no new opinions to combat, what occasion was there to

*nature and cure of Hospital and Jail Fevers, p.* 4. If Dr. Bancroft admits this to be typhus, he will, according to his doctrine, assume contagion as the cause; and, if he cannot find contagion, he will deny it to be typhus. At this rate, if the learned doctor can ever produce a single case of typhus, to the satisfaction of any man, who has a due regard to the nature of scientific proof, I will freely acknowledge my error, and make every atonement in my power to the world, for having endeavoured to mislead them, upon a matter of such high importance to their welfare. On the other hand, I trust it will not be too much to expect of Dr. Bancroft, and all others, who have adhered *bona fide* to the same doctrines, that they will imitate the truly magnanimous example of Dr Rush.

re-produce, at this particular period, all the hackneyed absurdities of the latter ages, in support of contagion in these diseases? Would not the hypothesis, in that case, have remained in sufficient security and preservation? I believe I may venture to say, it might have to this day enjoyed the most uninterrupted domination, if I had not ventured to set the example of calling its authority universally in question. Until proof of the contrary be given, I must take the liberty to maintain, that my "Dissertation on the source of epidemic and pestilential diseases," contains the earliest, and, I believe hitherto the only refutation of this destructive and gigantic error; and that not a particle of the credit is due either to those, who have claimed fragments of the doctrines for themselves, or on whom Dr. Bancroft has, with more generosity than justice, bestowed either expressly, or by insinuation, portions of the inheritance. If it were otherwise, there could have been no difficulty in referring to any prior refutations; and their authors would, in that case, no doubt have been named, particularly if they were of foreign nativity, or had been some time numbered with the dead.

With respect to Dr. Bancroft, it cannot be deemed very illiberal in me to presume, that he could not have had much objection to have seen me loaded with the opprobrium of the pernicious opinions, as he considers them, respecting the

cause of typhus and plague, which he fancies himself to have refuted, if this would not have been too clearly to indicate the original source of the doctrine, respecting the cause of yellow-fever, the credit of which, according to his imperfect exposition of it, he was ambitious of appropriating. Sensible of the indecorum of such a proceeding, to say the least of it, and seemingly in anticipation of the charge, to which he could not but be aware that he was eventually subjecting himself, this writer has artfully endeavoured to confound the right to the *title* of a discoverer of the truth, with the right to the *use* of the truth when discovered, by a misapplication of the common place truism of old Montaigne: "que la verité, et la raison sont commun a un chacun, et ne sont pas plus a qui les a dites premierement, qu'a celui qui les a dites apres." Nothing, indeed, can be more plain; and scarcely any thing less in need of illustration. But, it is also clear, that, the faculty of discoveries is not equally given, and that the credit cannot be equally due, to all men.

The fragment of truth, respecting the cause of yellow fever (it is literally a fragment; for the repeater of it is only right in what is *not* the cause of that disease), I might, in charity, have spared to Dr. Bancroft; were it not for the consideration, that, encouraged by this forbearance, other pretenders might lay claim, each to the

discovery, that some particular epidemic does not depend upon contagion, all of them equally unconscious with him of the only knowledge in the case; viz. that no epidemic disease whatsoever arises from that source.

The proposition in my original tract upon this subject, at page 2, states universally, "that no general disease, which is capable of affecting the same person more than once, is ever communicated by contagion." It is shewn that all epidemic diseases are capable of affecting the same person repeatedly—from whence it follows, as an inevitable consequence, that epidemic diseases never depend upon contagion. This is the precise nature of the doctrine, to which I explicitly claim a right, because no one before me has ever demonstrated, nor, to my knowledge, affirmed this comprehensive conclusion. Those, who have since taken small branches of this proposition, and given them as their own, I must therefore be allowed to consider as plagiarists, not even of the most modest description. And with respect to the persons, who may have, at different periods, previous to the promulgation of my doctrines, maintained, that some particular epidemics have not depended upon contagion; they are so far from having any claim to any portion of the credit of hinting or suggesting the discovery, that, the denial of contagion partially, is a virtual admission of its existence generally in

epidemic diseases. To attempt to trace to such partial, or local views, the origin of the comprehensive doctrine, now established upon the basis of irrefragable demonstration,—"that no epidemic disease is ever propagated by contagion," would be as unreasonable, as to attribute to such hints, as the letting of blood, or the feeling of the pulse, which were practised two thousand years ago, the credit of having suggested to Dr. Harvey his grand discovery of the circulation of the blood.

In like manner, it may be observed, in respect to my means of cure, that, if the petty larcenies of such delinquents were not detected and exposed, as many separate claimants might start up, as there were separate propositions, belonging to that part of my discoveries: and those detail plunderers, in decking themselves, like the jay in the fable, in borrowed plumage, might strip me successively of the credit of all the results of my researches and investigation.

But these sins of commission, and of omission, as they affect only the rights of the individual, are comparatively but a trifling grievance. It is the mischief to science and to mankind, if these principles be correct, that has arisen from negligence, or a want of comprehension, or a deficient illustration, in regard to them, or a combination of all the three, which constitutes the

evil now chiefly to be deplored. Had the injustice to the individual been greater, by a more general, but less imperfect clandestine adoption of them, the injury to the public would have been, in the same proportion, diminished.

Of the extent of the detriment, which has arisen to medicine, and to communities, from the delay, which has taken place, in the reception of the doctrines here promulgated, we may now be enabled to form some adequate estimate; especially in having entailed upon nations, in respect to epidemic diseases, the double evil of the prolongation, for twenty years, of all the pernicious consequences of the belief in contagion; and of the non-application, or mis-application, of remedies.

Whether the meliorations, in medicine, proposed, in 1796, and here confirmed, have not been hitherto rendered available to the public, because the members of the medical faculty have not seen them, or have not understood them, or have not chosen to adopt them, is a matter perhaps of little consequence now to determine. But why they have not seen them, or have not understood them, or have not chosen to adopt them, if it should appear that they were not originally wholly unintelligible, and that they have been always in an open course of circulation, they may deem it necessary to their own

justification to explain. For my part, confident of final success, from the certain, although slow operation of reason, I would not be understood, at this day, to deprecate one iota of the hostility of opponents; whose exertions, when I had not equal means of triumph, I always regarded without apprehension, and without respect.

There is another part of Dr. Bancroft's Essay, which demands some portion of my notice, not so much because it involves the most delusive opinions, concerning the cure, a matter by no means either peculiar or uncommon, but because it incidentally involves a claim for another person, which I believe that person, has also himself preferred, to a part of my medical discoveries. The circumstances are these. Dr. Bancroft, it seems, was one of the physicians to the forces in the West Indies, where the extraordinary mortality happened to the British army, in the year 1796, 7, and 8, which I have stated at page 14 of this Discourse. Dr. Chisholm, who has also written a book upon pestilential diseases, was, at the same period, engaged, upon medical service, in that country. He, with a few others, were strenuous advocates for the employment of mercury in yellow fever. The opposite, and most numerous party, of whom Dr. Bancroft appears to have been one, were no less strenuous against the use of mercury, and in favour of

blood-letting. Phlebotomy carried the day, and carried off the patients. Dr. Chisholm, in his publication, reproaches the friends of that practice with the great mortality, which he thinks it occasioned. Dr. Bancroft, without denying the charge, retorts upon Dr. Chisholm, "The mortality with which the medical officers of Sir Ralph Abercrombie's army are here reproached was certainly great, (and ever to be deplored); but it did not, in any instance, extend to 21 out of 26 patients, as happened to persons under Dr. Chisholm's care, after his *pretended* discovery of an almost certain remedy [1]." He also denominates this practice, Dr. Chisholm's "*strange and unpromising innovation* [2]."

With respect to this dispute, which might in legal phraseology, be entitled "the deleterious effects of blood-letting, *versus* the deleterious effects of the mis-application of mercury," it may be observed, that it cannot be of much consequence to the public, since so great a mortality was destined to happen to the army, to be informed whether most of it was occasioned by one species of mal-practice, or by another; a matter, which, unfortunately, it is extremely difficult, with respect to particular cases, where so much depends upon the relative intensity of diseases,

[1] Bancroft's Essay, p. 767, note. [2] Ibid.

correctly to ascertain. In general, however, it may be pronounced, that, in diseases of high degree, like yellow fever, it is difficult to suppose any degree of mis-application of mercury, which can be so deleterious, as the most ordinary abstraction of blood, occasioning always a direct and positive increase of the pre-existing malady: and, if it be true, that, under the mis-application of mercury, Dr. Chisholm lost 21 out of 26 patients, it is probable, that, under that degree of abstraction of blood, which has of late, been in such cases, enjoined, he would not have preserved one of the same patients alive. To speak more precisely, no mis-application of mercury can, whilst the pre-existing disease remains unremoved, produce a disease more dangerous: but, the diminution of the blood, without the possibility of removing, adds directly to the force, and increases the danger from the original malady. These positions are severally illustrated in treating of the cure.

Fertile as is the subject, I must here refrain from enlarging upon it. But, in contemplating the prodigies of blood-letting, which, even in these enlightened times, continue to be performed, in almost all quarters of the globe, with such distinguished eclat, and especially in calling to mind the manner in which Sangrado has been recently so unmercifully eclipsed upon his native soil, it

is quite impossible to avoid recollecting the dialogue between Gil Blas, and his secretary, Scipio, as they came in sight of Valladolid, and almost equally impossible to resist quoting it. "At sight of this last place, I could not help heaving a profound sigh; and my companion, who perceived it, asking the cause, 'Child,' said I, 'I practised physic a long time in this city; and my conscience upbraids me with it at this moment! Methinks all the sick people whom I killed come out of their tombs, and seem ready to tear me in pieces.' 'What a fancy is this,' said my secretary, 'truly, Seignior de Santillane, you are too good. Why should you repent of having laboured in your vocation? Observe the oldest physicians; do they feel any such remorse? No, sure: they still go on in their old course, with the utmost tranquillity, throwing the blame of all fatal accidents on nature, and claiming honour from every lucky event'

"True," said I, "Dr. Sangrado, whose method I faithfully followed, was a man of that character. Though he saw twenty people die daily upon his hands, he was so well convinced of the excellence of bleeding in the arm, and plentiful draughts of warm water, which he called his two specifics in all kinds of distempers, that, instead of suspecting his remedies, he believed that his

patients died because they had not drank, and been blooded enough[1]."

The happy ridicule of Le Sage falls much short of the extravagance, to which this most barbarous and most deleterious practice is now carried, in real life, in many countries, especially in the treatment of those diseases, in which it is most of all injurious. We are told by Le Clerc, after Pliny, that it was first adopted by men, in imitation of the hippopotamus, or sea-horse[2], an origin truly appropriate: and, in epidemic diseases, we learn from Sydenham, that it was first employed largely, in this country, at Dunstar Castle, in Somersetshire, by an ignorant, or insane soldier[3].

It is no proof of the propriety of this practice, that it has been followed, in some diseases, even

[1] The Adventures of Gil Blas of Santillane, Book XII. Chap. I.

[2] The History of Physic, by Daniel Le Clerc, M.D. Translated by Dr. Drake, &c. p. 68, and 71.

[3] *Swan's Sydenham*, p. 83. The credit given to tales of this description has been of extreme injury to medicine. But when we see such men as Sydenham lend an ear to them, it ought not to surprise us that they have obtained so easy and so general a credence. As, in respect to the cause of epidemic diseases, it is shewn that they never depend upon contagion, so, in respect to their treatment, it is demonstrated that the abstraction of blood can never contribute toward the cure.

by Hippocrates, Sydenham, and Brown; any more than it is a proof of the truth of the existence of contagion in epidemic diseases, that it has been received by all the Christian world, from the moment of its promulgation, on the authority of the head of the Christian church. Confidence in the efficacy of blood-letting, as is shewn in treating of the cure, is, if possible, a greater opprobrium to physicians, and scarcely less injurious to mankind, than the belief in contagion.

But my business, at present, is more particularly with what Dr. Bancroft calls Dr. Chisholm's "strange and unpromising innovation." This gentleman is stated to have been a practitioner at Grenada, in the West Indies; and he states himself to have there employed mercury successfully in yellow fever in 1793. Upon this allegation Dr. Bancroft observes, "Nor did I find that his boasted success in that island, from the use of mercury in the yellow fever, had been known there, until his *printed accounts* of it had *arrived from Europe, nor that it was believed when thus made known*[1]." At what time the printed accounts mentioned might have arrived in the West Indies, or at what period the *abuse* of mercury might have been generally extended

[1] Essay, p. 768.

to the Islands, and to America, I am not sufficiently informed. But I can mention the precise period, at which mercury was first employed, *upon principle*, in yellow fever, because it happened to be under my own direction. This took place early in the year 1790. The first patient was an overseer of the name of Macmullan, on the estate of my late worthy friend Dr. Hector Maclean, of Russel Hall, in the parish of St. Mary, in Jamaica. He had been given over, as the phrase is, by the medical attendants of the plantation, Messrs. Logan and Bennet, of Port Maria, partners. Being on a visit to Dr. Maclean, at the time, I represented to him the good effects which I had derived from the use of mercury, in other fevers, (the practice was introduced by me some time before, and was then well known in the East India Company's service; facts which I think it necessary to state, for the information of those who may be, or have been, preparing claims in their closets,) requesting, that, as he, and the gentlemen who were in ordinary attendance upon the patient, seemed to be of opinion that nothing farther could be done for him, according to the received rules of the art, I might be permitted to try a method, which I had upon so many occasions found efficient. To this proposition, Dr. Maclean, who had been himself an eminent practitioner, but, having relinquished

the profession, was living upon his estate, acceded with a liberality which I had not always met with from physicians of established reputation. The result of the treatment employed in this case, extraordinary as it was considered, was widely circulated throughout Jamaica, almost the whole of which island I travelled over, in the course of that year, in the interval between two voyages to the East Indies. And this, I do not hesitate to affirm, was the real origin, in as far as the *name* of the agent is concerned, of what was afterwards denominated "the mercurial treatment" in America, and the West Indies; whilst, for the employment of the same agent, three years afterwards, in the island of Grenada, upon grounds wholly erroneous, and in a manner altogether imperfect, and consequently inferiorly efficient, although certainly not so opposite to all principle, as blood-letting, nor so distant from right principle as the other parts of the ordinary practice of medicine, credit is claimed by Dr. Chisholm, as if it were an idea of his own; as credit has been claimed by other persons, and in other places, for other branches of the same doctrines, in like manner imperfectly understood. A sketch of Mac Mullan's case is to be found at page 8 of my Treatise on the action of mercury upon living bodies; in which are also shewn the manner, and the grounds, upon which I extended

the application of that remedy, to many diseases, in which it had never before been employed, excepting upon the erroneous principle of an evacuant, or in which it had always been considered pernicious.

I must therefore take to myself whatever blame, and consequently whatever credit, may attach to this "strange and unpromising innovation," having been the first to introduce such a mode of employing mercury, as to cure yellow fever, as well as other fevers, and various acute diseases, in which it had never before been employed, excepting for the purposes of evacuation, and without occasioning any new diseases; as well as the first, who applied that remedy, in those diseases, in which, under the erroneous notion of a specific, it had, to the destruction of mankind, been most improperly administered, for hundreds of years, in a manner, by which it cures the original diseases, without substituting any new maladies. This I had done, both in the East and in the West Indies, for several years preceding the date long afterwards assigned by Dr. Chisholm to *his* discovery in Grenada.

This much I have thought it necesary to state here, not so much in assertion of my rights, which are easily capable of being at any time substantiated, and which it is not in the power of any ephemeral pretender to obscure; but, in

order to throw down the gauntlet to all the secret or anonymous defamers of the principles for which I contend, whose arguments, if they will commit them to the decision of public opinion, instead of addressing clandestine unfounded and irrelevant denunciations to the government offices, and to the great public body, whom I have the honour to serve, and who have always done me the justice to treat the efforts of my traducers with the contempt they merited, I shall be happy to have the opportunity of encountering in the face of day.

But, not less willing to bury past grievances in oblivion, than to contend in a just cause, I now solemnly call upon the members of the medical profession, without prejudice, or pre-possession, to weigh, upon their proper merits, the important matters, which I here re-submit to their perusal. They are of a nature to impose the indispensible obligation, upon every cultivator of medical science, of bestowing upon them, a candid, a diligent, and an attentive examination; of avowedly receiving and sanctioning them, if correct; if mixed, of separating the sound from the unsound parts; or, if wholly erroneous, of openly rejecting and refuting them. According to my idea of moral obligation, there cannot, in such case, be any choice.

These innovations are confessedly important, and of extensive applicability. They are either

correct, and capable of effecting much good; or erroneous, and capable of effecting much mischief. They have nothing negative in their nature. The problems cannot but be deemed of extraordinary importance to mankind.—1. Whether an immense number of human lives be not the annual sacrifice to a single delusion? 2. Whether there be not as great a sum of sickness, and mortality, constantly occasioned, by the misapplication of agents employed as remedies, as by the ordinary causes of disease? and 3. Whether upon the right decision of these problems, and its consequences, there do not annually depend, not thousands, but millions, of human lives? Concerning the fate of such a discussion, I do not see with what consistency conscientious men can permit themselves to be either silent, or inactive.

If, upon the question of contagion in epidemic diseases, there should be, amongst the medical faculty, persons, who, from indolence, may be disposed to take for granted the affirmative, without examination, but who would spare themselves the misery of subsequent unavailing regrets, let them peruse, and ponder over, the solemn, and I would add righteous recantation of Dr. Rush.

In respect to the treatment, in order to judge between the consequences of the practice, which

has generally prevailed, and that which I here recommend, let them, without referring to the whole circle of human ailments, or the entire range of the materia medica, only well contemplate the misery and mortality, which have been occasioned by the misapplication of only one remedy, in only one disease, for the space of three hundred years. In the general ignorance of the mode of operation of mercury only, strange as it may at first sight appear, together, in this country, with the mal-organisation of the different branches of the medical profession, in their relations to the community, will be found, if I be not greatly deceived, some of the principal causes of the flourishing state of quackery, and of the ruined state of the constitutions of those, who confide in its delusive aid.

Respecting my silence, concerning doctrines of such great importance, and extensive applicability, during the long interval, which has elapsed, since their first promulgation, although it cannot be very material to the public to know, by what particular circumstances it may have been occasioned, it appears proper that I should enter into some explanation: And first let me state what it was *not* owing to: It was certainly owing to any thing, and I trust this work will fully bear me out in the assertion, rather than a

distrust of the goodness of my cause. Amongst others, there was one consideration, which had great weight, in determining my decision to remain silent. Upon viewing the subject under every possible aspect, it did appear to me more than probable, from the inveteracy, as well as the universality (amongst Christians as I have since ascertained) of the belief in contagion, that all farther efforts, to elucidate, or to enforce, my opinions, respecting the cause of epidemic diseases, would be unavailing, until my reasoning should be invested with the sanction of that practical knowledge, in what is deemed the highest degree of pestilence, which I had long sought an opportunity of obtaining, and have at length been so fortunate as to acquire.

But, although, under these circumstances, I deemed it unavailing to persevere in attempting to procure for such heteredox doctrines, farther publicity, or an unbiassed examination; yet, I never wholly lost sight of the subject: nor did I, in the intervening period, omit any opportunity of prosecuting, for my own satisfaction, the researches, which I had begun in early life concerning them, whether by actual experiment, reasoning and induction, the perusal of books, or verbal enquiry. These pursuits, indeed, were often unavoidably interrupted, by other more indispensable avocations. But the opportuni-

ties, which have occurred to me, of observing, and treating, the epidemic diseases, of various countries, have, upon the whole, been such, as do not fall to the lot of many: and personal experience of the plague of the Levant, seemed alone wanting, to enable me to bring my enquiries to a regular conclusion.

This being now obtained; my original opinions being confirmed by a great variety, I might say a superabundance, of new proofs; and the fraudulent origin of the doctrine of contagion being demonstratively traced in the records of history; I have waved all considerations of personal convenience, or interest; and have not suffered the intervention of a moment's delay, beyond what has been absolutely necessary to give to the fruits of my experiments and enquiries, as well as to the materials collected from foreign sources, a due method and arrangement, in bringing them, thus re-inforced, again before the world.

By the gratuitous publication of the results of my researches, I am aware that I am placing myself in a very singular predicament; that, whilst I am proffering benefits of no ordinary description to the community, I may be represented as one, who is soliciting personal favours; and, that, in making a free offering of my discoveries to the world, I incur the risk of

being deprived, by some stranger, alike to the danger of my investigation, and to the toil of my inferences, of the reward, although he cannot deprive me of the credit, due to my services. Yet, being placed between the alternatives of incurring the risk of this sacrifice, or of retaining useful discoveries undivulged, I have not allowed myself, for a moment, to hesitate.

For the purpose of being freed from all species of restraint, in announcing opinions, which I consider to be of so much importance to mankind, I have felt it my duty to assume an attitude of entire independence upon the medical profession. Unskilled in the ways of policy, if I have followed the road pointed out by integrity and decision, perhaps it will not be deemed the most unadvised course. I am sensible, that, were I to content myself with refuting only individual portions of the errors, which I attack altogether, or of advancing only individual propositions of the doctrines, which I maintain as a whole, such portions would be received more readily, and the personal obloquy arising from their advancement would not be so great. But, besides, that, in such a case, this would be shamefully to compromise with my conscience, it would be to forego the advantages, which a perfect unity of system confers. By a temporary renunciation of all views of professional

emolument, I secure to myself a complete independence of the prejudices of patients, as of the prejudices of the medical faculty, and the liberty of freely uttering professional truths, without the risk of injury from the utmost malevolence of the traders in medical spoil.

In adopting this resolution, I have had experience for my guide. Beside the slander, in excuse for the rejection of discoveries, necessary to be poured upon the discoverer, it was an effect of the first promulgation of my doctrines, that it left me no chance, in this country, as the branches of the profession are divided and regulated, of a fair participation in its emoluments; whilst, in other countries, in which I have resided, where apothecaries are not the conscience-keepers of families, where there are no such lusus naturæ as pharmaceutic prescribers, practice has spontaneously flowed in upon me, according to the utmost extent of my pretensions, supposing them to be not inferior to those of the ordinary run of physicians.

Of these things I would be understood to speak with total indifference, and as matters in which I do not feel any personal concern. I never have consented to exercise the art of medicine, but in perfect independence of the prejudices of patients, on the one hand, and of those of their medical attendants, on the other:

and, whenever I resume it, if that should happen, it shall be upon no other principle.

But, should this renunciation of my professional views, although not a trifling sacrifice, prove the only one which I shall have to make to the promulgation of these doctrines, I shall not complain: and it will be a source of infinite gratification to me, to find the present medical generation so much more liberal, than the cotemporaries of Sydenham, as not to justify my entertaining similar apprehensions, with those expressed by that illustrious physician, in presenting the public (a subject very similar) with the history and cure of acute diseases: "And though in executing this design, I am sensible I shall expose the fruit of my labours for the best part of my life to the lazy and ignorant, yet I am too well acquainted with the disposition of this degenerate age, to expect any thing but censure and contumely in return; knowing that *I should have gained more reputation by advancing some trifling and useless speculation*[1]."

In case I may not have already expressed myself with sufficient explicitness, I desire again to be understood, as meaning, that, although this work, should, as a whole, be regarded as

[1] Swan's Sydenham, Author's Preface, p. xxi.

constituting a production entirely original, I have in what respects the cause of epidemic diseases, sedulously consulted the opinions of the great father of Physic; in what regards their history and cure, those of the illustrious Sydenham; and in what regards the general laws of life, those of the celebrated Brown. The doctrines of those eminent philosophers, and physicians, in as far as they are conformable to nature, I have endeavoured to preserve, to restore, or to amend. But, whilst I revere, as I ought to do, their labours; my own have been invariably directed by that admirable maxim of the Latin poet, which ought to guide all scientific researches, and to be always held particularly sacred in medicine;—"nullius addictus jurare in verba magistri." Following this maxim, as often as those great masters have appeared to me to depart from nature, I have not scrupled to depart from them. I consider them as the polar stars of medicine, in their respective departments. And could I be assured that, in such company, my name should descend to posterity, it would more than compensate the injustice of a generation.

It may here be remarked, as a matter of curious contemplation (and an enquiry into the reasons might well employ the talents of an aspiring young Physician), that, whilst, in Greece, Hippocrates, in his life, enjoyed the honours justly

due to his eminent services; in England, Sydenham experienced the bitter hostility; and in Scotland, Brown the persecution of his medical cotemporaries. Is it, that the spirit of monachism, which, in the later ages, in the countries of Europe, seems to have pervaded the institutions of medicince, had, in ancient Greece, no existence; and, that, in proportion as his doctrines assailed fewer interests, prejudices, and passions, and his station in society was more unassailable, this spirit was both less vindictive, and less injurious, in the case of Sydenham, than in that of Brown? In persecution, there is a marked, though tacit, acknowledgment of superiority, which is calculated to assuage the wounds, which its malignity inflicts. His taste, who prefers mediocrity, even accompanied by the execration of mean competitors, with the consciousness of having well served his fellow-creatures, to the highest gifts of fortune, if obtained by an acquiescence in error, or the exercise, in what concerns the health and lives of men, of a calling, whose rules are founded upon hypotheses or conjectures, will be certain to meet with ultimate applause, even from those who have got but a bad taste of their own. And, if those persons, who may for a while, have succeeded in calumniating his actions, or frustrating his views, should be able to derive satisfaction from a retro-

spect of their conduct, *he* will be able to enjoy a triumph, both more noble and more exquisite, in the consciousness of having accomplished objects of general importance to mankind.

In ordinary times, the progress of improvement in medicine may be somewhat accelerated, when it happens to come recommended from hands that have distinction, promotion, or favours, to bestow; or it may be momentarily retarded, when it happens to come prejudiced through channels obnoxious to persons so endowed. But, where freedom of discussion exists, such adventitious obstacles, or support, can only be of transient operation: and doctrines will ultimately prevail according to their intrinsic merits. Such, I think, is the actual state of the age. The spirit of monachism, and superstition, which, for so many centuries, has held medicine spell-bound, is dissolved for ever: and truth alone can sustain the ordeal of the disquisitions of the nineteenth century. For my part, whilst I have health, no discouragements shall deter me from continuing, as in duty bound, to expose the senility of the existing dogmas in medicine; to search after new truths; to divulge those which I may discover; and to assert my right of priority in their promulgation.

If there be yet physicians, who are ready, without examination, to oppose innovations, or

ancient doctrines restored; or who consider *their private censure* as equivalent to a *public refutation;* I venture to assure them, that they will find themselves exceedingly mistaken, both in respect to the extent of their own influence, and to the actual state of knowledge, in entertaining the vain expectation of being able to continue to restrain the medical intellect of the nineteenth, within the limits of the dogmas of the sixteenth century; to prolong the reign of the pernicious delusions, respecting contagion, issued from the Vatican, in the year 1547; or to keep any branch of the interesting science, which relates to the health and life of man, much longer in leading strings.

For the promulgation of principles, not at all times, or in all countries, equally applicable, it may, at first sight, be thought, that I might have chosen a more fit occasion: and, certainly, had it been my plan, by embracing a favourable disposition of the public mind, to procure, at any rate, a momentary attention, and an extensive circulation, for my work, the period, and the seat, of a reigning pestilence, would have been a more appropriate æra, and scene, of publication. But I doubt, whether such a concurrence of circumstances, although it might be useful to the work, would not be unfavourable to its principal object. Considering with how much more efficiency an investigation, of this nature,

is capable of being conducted, amidst that coolness of self-possession, that regulated state of the passions, which the absence of pestilence admits, than amidst the dread inspired by the actual progress, or near approach, of a destructive malady ; and, again, how much more dispassionately the results of such an enquiry are capable of being examined, and appreciated, in countries, in which the fewest prejudices, upon the subject, prevail ; I am decidedly of opinion, that, with a view to these paramount objects, there could not be a period, more favourable, than the present, or a country, better adapted, than our own ; whilst we are free from the presence, as well as from the apprehension of any immediate attack of pestilence, and when every fact, and every argument, will, consequently, be allowed the weight, due to their intrinsic merit. In all countries, during the actual presence of pestilence, the interest is too intense, to admit of dispassionate decision : In a Catholic country, my inferences would be regarded as heresies ; and in a Mahommedan, unworthy of consideration.

How far I have properly executed my design ; whether I have succeeded, in restoring, to light, truths, which have lain buried in oblivion, for some hundreds of years ; or, like my predecessors, since the revival of learning, involved the sub-

ject in still greater obscurity, must be left, for others, to decide. Neither can I be a judge of how far, my expectations of the benefits, to be derived to mankind, from the restoration of sound doctrines, respecting the cause of epidemic diseases, and from the introduction of a conformable mode of prevention, and of an efficient method of cure, have been restrained within reasonable bounds. This much, however, I may be allowed to affirm, that, if I have been only dreaming, my dreams have been unusually consistent. And, if that should turn out to have been the case, truth being my principal object, I shall be ready to esteem much more highly, the person, who may condescend to undeceive me, than him, who, by lending his countenance to my reveries, shall contribute to confirm my delusion.

With respect to the language, as it has not been my design to adapt this work to the apprehension of particular classes only, I have, as much as possible, studied simplicity; avoided technical phrases; and endeavoured, without rendering it entirely unworthy of the more learned reader, to make it also intelligible to every capacity.

In the arrangement, I have endeavoured to observe that order, which has appeared the best calculated to give to the reader, the clearest, the

fullest, and the most impressive, idea of my meaning. The elucidation of the origin, the erroneous nature, and the injurious consequences of the doctrine of contagion, I have thought right to preserve entirely distinct from the narrative of my researches in the Levant, and from the discussion concerning the nature and cure, and the cause and prevention, of epidemic and pestilential diseases; both because the subject can be more conveniently treated in this way, and because, in effect, they have no necessary connection with each other. Errors, which are to be exploded, have nothing in common with truths, which are to be rendered manifest.

In treating of a subject of such distinguished importance, it has been my aim, that, every fact, and every argument, should be fully stated; and, that, even at the risk of occasional repetition, they should be placed in such various points of view, that every reader might be able to find something calculated to arrest his attention, to captivate his fancy, or to convince his understanding.

# PART II.

## REFUTATION OF THE DOCTRINE OF CONTAGION.

## BOOK I.

### DISTINCTION BETWEEN DISEASES, CONSIDERED IN RESPECT TO THEIR CAUSES.

### CHAP. I.

*No distinction between diseases in respect to their nature —in respect to their causes, they may be divided into three classes—Of epidemic diseases—they are occasioned mainly by the air—Contagion almost the sole power which cannot concur to produce or to aggravate them—the existing nomenclature improper—ideas respecting a new one.*

Considered in respect to their nature, there is on ground of distinction, as will hereafter be shewn, between epidemic and pestilential diseases, those which depend upon contagion, and those which are produced by other powers.

But in respect to their causes, the distinction between the morbid affections of organs is of

the utmost consequence, in as much as upon it depends the means of their prevention.

Diseases may, upon this principle, be divided into three classes: 1. Epidemic and pestilential diseases:—2. Those which depend upon contagion:—And 3, all affections not comprehended under these two divisions.

Epidemic or pestilential diseases are such as affect many persons at the same time, being produced mainly by an exciting power, the air, of the most diffusive operation.

Epidemic diseases, then, in their utmost latitude of acceptation, may be defined to be, such deviations from health as depend upon the undue action of the atmosphere, comprehending all the intermediate degrees of affection, between the slightest catarrh, and the most destructive pestilence.

Towards producing an epidemic, or pestilential disease, to the diminished exciting power of the atmosphere, are always superadded, in their various combinations and degrees, the influence of heat and moisture, soil and situation, food and water, corporeal labour, the passions and emotions of the mind, and in Christian communities, the consequences of the belief in contagion.

It cannot fail to be regarded as extraordinary, that, of all the powers, which are, or can be

applied to the living body, that to which, for the last 270 years, they have been usually attributed, is the sole agent, which cannot possibly concur to produce, or to aggravate an epidemic.

The effects of the action, in its different degrees and modifications, of a power of diffusive and constant operation, which is the appropriate stimulus of the grand organ of respiration, and by which all the external parts of the body are perpetually pressed and enveloped, must necessarily be infinitely various. It is, directly, or indirectly, the source of a great proportion of all the maladies, which afflict mankind. Its slighter consequences, which would not of themselves, prove dangerous, frequently become the foundation of diseases, which prove mortal: those, which already exist, it aggravates; and renders some fatal, which would otherwise terminate in recovery.

The vicissitudes of the atmosphere are a principal cause of consumption, which is so fatal in England, as to be considered a national malady. This disease, which is also common in Constantinople, and throughout the Levant, is there supposed, amongst Christians, to be contagious, and by some dreaded nearly as much as the plague itself. This well known cause, has, of late years, I am very strongly of opinion, obtained a powerful ally, in the almost universal

misapplication of mercury, by which consumption is rendered more frequent, severe, and mortal, and slighter diseases often converted into consumption.

Popular tradition, then, seems justified, in regarding common colds as the foundation of almost all the ailments of mankind; and the great father of physic, in considering the air as the cause of almost every malady.

It would not be necessary, if it were practicable, to enter into a formal enumeration of the various affections, which, under this view of the subject, properly belong to the class of epidemic diseases. Amongst others, which have not hitherto been so considered, we may, for reasons which will be rendered sufficiently obvious, reckon scurvy, which may be truly regarded a maritime pestilence. The species of pestilence, which occurs in the Levant, is known by the name of plague. Epidemic diseases, pestilential diseases, and plague, although due regard shall be paid, when necessary, to the distinctions which really exist between them, shall, in this work, be frequently employed as synonimous terms.

As, upon the principle of "majus constinet in se minus," a knowledge of the nature of maladies, of high degree, virtually includes that of all minor analogous affections, it will be sufficient here to treat fully of those epidemic diseases,

which are strictly pestilential: and plague, which comprehends every variety, and degree of affection, common to this class of maladies, may be aptly regarded as the representative of the whole.

The yellow fever of the West Indies, and of America, the fevers of Bengal, Batavia, Bencoolen, Bulam, Cadiz, Gibraltar, Andalusia, Malta, Walcheren, and Leghorn, &c., &c., &c., (for so the epidemics, which have occurred at these places, have been most improperly denominated) as well as every variety of remittent, and intermittent fever, which will all be found to be only modifications of one, and the same disease, produced by modifications of the same cause, and yielding to modifications of the same remedies, shall, with minor epidemic affections, be only occasionally treated of, in the way of illustration [1].

I may here observe, that, the appellations, from local circumstances, or particular symptoms, which these and other fevers, have obtained, as if they were diseases differing in their nature, are as unscientific and absurd, as if the febrile affections of different individuals, from their varying, in symptoms, and degree, should be distinguished by the proper names of the persons, who are

[1] A Mediterranean fever is now added to the list. *Burnet*

labouring under them, as a "John Fever," a "Tom Fever," a "Bill Fever," &c. Thus, in India, there is a "Hill Fever[1]," from its occurring in the mountains, and a "Jungle Fever[2]," from its occurring in the woods; and, in England, a "Fen Fever," from its occurring in the marshes: and this nomenclature we often find formally employed in medical dissertations! With equal propriety, might we speak of a "River Fever," and a "Plain Fever," a "Sea Fever," and a "Land Fever," a "Night Fever," and a "Day Fever," a Dinner Fever," and a "Supper Fever," cum multis aliis.

For these, and other reasons, I trust I shall stand excused for disregarding, in this work, the arrangements of an artificial, and misleading nosology; and for considering diseases according to the classification of nature only, still acknowledging the necessity, in order to be generally intelligible, of continuing to employ, in most instances, the generic names, however inappropriate, by which diseases have hitherto been usually designated, and known.

At present, I feel, that it would be premature, and in the imperfect state, in which the science of medicine still remains, might be injurious

[1] So called on the coast of Coromandel.

[2] This term is used in the northern parts of Hindostan.

hastily to attempt a nomenclature of diseases, founded upon scientific principles: although I am, by no means, without hope, that the period is not far distant, when this attempt may be made, with some prospect of success. And, if it cannot be done immediately with advantage, it is, I think, rather because some considerable preparation of the public mind, and some degree of general consent amongst physicians, may be previously necessary, than that the principles themselves are either obscure, or ambiguous.

## CHAP. II.

*Difference between contagious and epidemic diseases—contagion and air agents of dissimilar properties—A contagious atmosphere, or an atmospheric contagion, nonsense—A disease, commencing in contagion, is never, in its course, propagated by other means—And, a disease, in its commencement, produced by other means, never, in its progress, becomes contagious—Laws of contagious diseases—their phænomena uniform—general contagious diseases incapable of affecting the same person more than once—this circumstance sets boundaries to infection—origin of contagious diseases unknown—errors of Dr. Mead.*

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As in the delusive doctrines, respecting the cause of epidemic diseases, the origin of which is traced in succeeding chapters, not only the effects of the air, and of contagion, but these agents themselves, have been completely confounded, it is necessary here to shew, that they are powers dissimilar in their properties, and that the diseases, which they produce, are, in respect to their means of prevention, to be very differently regarded.

The properties of contagious matter, in occasioning diseases, are by no means obscure. Those which they are capable of producing,

are determinate; and cannot be produced by other agents. The virus of a contagious disease is as distinct from the air, as opium is from mercury. It will produce its appropriate effects, in the most pure, as well as in the most impure atmosphere. These two powers are indeed, so distinct, that, if men could live wholly without air, it is certain that contagion would, in its absence, produce its ordinary phænomena.

The diseases, which the air is capable of producing, are, on the contrary, from its diffusible operation, and from the great variety of degrees in which it may be deteriorated, of almost infinite variety, both in respect to phænomena, and intensity. This is exemplified in the diversity of affections, which may take place, from the head-ach, which is produced by the air of a crouded play-house, to the mortal disease, or the instantaneous death, which is the effect of a more adulterated atmosphere, as that of the grotto del Cani, or the black hole of Calcutta.

The diseases so produced, however, are wholly incapable of reproducing themselves, by contact of the persons affected, with persons in health; a mode of propagation, which is the true criterion of, and is indispensable to, a contagious disease.

With respect to the maladies occasioned by casual immersion in noxious air, as in jails,

wells, and subterraneous passages, the circumstances are so unequivocal, that delusion cannot readily take place; they are consequently not liable to be mistaken for contagious diseases, although since the middle of the 16th century even this has been known to happen, as in the case of certain jail distempers.

But the diseases, which depend upon the qualities of the external air, and affect considerable portions of a community at the same time, have, under the influence of this delusion, been more subject to the misinterpretation of their cause. When people are seized in a certain succession; when the number of the affected becomes considerable, and terror spreads, the mind revolts from the task of accounting for this succession, by the operation of natural causes; and what the superstition of former ages imputed to the anger of the gods, has, in modern times, been attributed, by a superstition still more degrading, to a specific contagion, propagated, from the persons affected, to those in health. And, it may here be remarked as curious, that, whilst, of the former superstition, physicians did not participate, in the latter they have taken the lead.

A contagious, or infectious atmosphere, it must be obvious, is, strictly speaking, nonsense: and the employment of such misleading

terms cannot but be extremely detrimental to science.

Hence, it must also be obvious, that certain propositions, so frequently insisted upon, by writers upon epidemic diseases, as necessary to the consistency of their doctrines, are no less absurd than erroneous. One of these, to which I particularly allude, is, that a disease, which, at its commencement, depended upon the state of the air, as affected by vegetable exhalation, animal putrefaction, or other agency, does in its progress, sometimes, become contagious: another, that a disease, which was originally produced by infection, does, in its progress, sometimes cease to be contagious, or becomes capable of being propagated by means of quite a different kind.

There is, in fact, no more foundation in reason, for attributing epidemic diseases to contagion, than any of those diseases comprehended in the third class.

"If you and I are exposed to the rain," observed Dr. Radcliffe facetiously, upon being asked his opinion respecting contagion, as applied to epidemic maladies, "we shall both get wet; but it does not follow that we shall wet one another." If a great many persons be so exposed at the same time, besides getting all

wet, some of them will get cold, others fever, others diarrhœa, and others rheumatism; but they do not *infect* one another.

In the same manner, if any number of persons drink wine, or use mercury, in the manner in which these agents are commonly employed, they will, in due time, get inebriated, or in a state of salivation: but this does not happen by the propagation of these affections from the one to the other. If they use these agents in equal proportions, they will still become affected at different periods, some sooner, and some later, and some not at all to a morbid degree.

Just so does it happen with respect to the operation of a noxious atmosphere, in an epidemic season: and nothing but the habit of thinking, in which we have been for ages, upon this subject, could make it appear less ridiculous to attribute the effects of air to contagion, than the effects of wine, or of mercury.

Let us for the sake of comparison, examine the leading laws of contagious diseases.

They are either general, or local. Of the former, small-pox, and meazles; of the latter, lues venerea, and the itch, are examples. A general contagious disease never affects a part, without producing general derangement of the functions. A local contagious disease, at its

commencement, affects a part only, without producing, excepting in its progress, general derangement of the functions.

It is a peculiar and distinguishing law of general contagious diseases, that they are not capable of affecting the same person more than once; whilst local contagious affections, like all those of the first and third class of diseases, are capable of affecting the same person repeatedly.

It would follow as a corollary from this proposition, that all general diseases, which are not capable of affecting the same person more than once, must depend upon a specific contagion. But, owing to the impossibility of obtaining this negative proof, in particular diseases, the *fact* of the *non-recurrence* of the same disease, in the same person, has been often confounded with the *principle* of its *incapability of recurring;* and hence much confusion has arisen, and many erroneous conclusions have been formed. Upon such grounds, do certain medical writers still persevere in attributing scarlet fever, the epidemics of Gibraltar, and some others to contagion.

But, if this be a misleading guide, there are sure criteria in abundance, to determine to what class of diseases any particular malady belongs. According as it appears subject to the laws of epidemics, or to those of contagion, it must take its station in either of these classes; or, if not

subject to the laws of either, it must belong to the remaining class. Respecting general contagious diseases, there can be no difficulty. They may always be recognised by the determinate periods of their phænomena, and the notoriety of their mode of propagation.

It is the incapability of affecting the same person more than once, which, in general contagious diseases, sets boundaries to infection; and could alone, where no precautions are taken, prevent communities from being extinguished.

The phænomena of specific contagion, as in small-pox, meazles, lues venerea, and the itch, always observe a certain uniformity, and are easily distinguished. They differ only according to the power of the virus applied, and the previous excitement of the patient, respectively. The distinguishing symptoms of small-pox, for instance, have so complete an uniformity, that there is hardly a possibility of confounding it with any other disease, notwithstanding that almost infinite variety of degrees, which may take place, from the most confluent kind, to that which has the smallest number of pustules.

In all general contagious diseases, the periods, after receiving the infection, at which their appropriate symptoms are manifested and disappear; as well as the periods, at which persons affected begin and cease to be capable of com-

municating the infection to others, are determinate. Even the periods of recovery, and of death, are subject to a certain degree of uniformity [1].

Diseases, which are produced by a specific contagion, have never been known to depend upon other causes; and diseases, which depend upon other causes, have never been known to be produced by a specific contagion.

Persons of both sexes, and of all ages, ranks, professions, constitutions, and modes of living, are, with the limitations hereafter mentioned, alike liable to be affected with diseases depending upon a specific contagion, in all situations, seasons, temperatures, countries, and climates.

Those who have already had the disease, if it be a general one, and those, who are labouring under a disease superior in degree to that which the contagion is capable of producing, are alone exceptions.

The origin of contagious diseases, like most other origins, seems to be beyond the power of human research. Nor is the enquiry, in a medical view, of any importance. However the contagious virus of any disease might have been first generated; in whatever country it might

[1] In the distinct small-pox, death generally happens on the 8th, and, in the confluent kind, on the 11th day of the disease. *Swan's Sydenham, p.* 102, 3.

have first appeared; or, in whatever manner it might have been afterwards propagated to others, if we could lay down a distinct chart of its proceedings, we should be little the wiser. This might, indeed, gratify curiosity; but it would not aid us, in the smallest degree, toward discovering better means of prevention, or of cure. In following the progress of these maladies, then, as fancifully described by Mead, and other writers, we can only lament the useless waste of talent, which might have been employed in researches of real value. "Small-pox," says he, "is, in reality, a kind of *plague*[1]." He might as well have said plague is in reality a kind of *small-pox*. Certainly, no two diseases can well be more dissimilar. "*Produced*, and *nourished*, like all other pestilential diseases, in the burning climate of Egypt, it afterwards spread all over Europe, and Asia, by means of the reciprocal commerce of nations, but particularly by means of that famous war with the Saracens, which was carried on, during the 11th and 12th centuries, under the name of holy war.....Meazles is also another *plague* of the same kind (and plague of course a kind of *meazles*) originating in the same climate.....They were not remembered in Europe, before the expedition of the Moors

[1] A short Discourse concerning Pestilential Contagion. Preface.

into Spain. But after the holy war, they spread, and propagated themselves in every country."

Some part of this may be correct. But if it were all true, it would be nothing to the purpose. It could not, in the smallest degree, improve our knowledge of those diseases. The statement, however, is full of error, and fraught with delusion. Besides confounding contagious, and non-contagious diseases, and stigmatising Egypt, as the common parent of the former, it is grossly deficient in historical accuracy. In Asia, where inoculation for the small pox is practised every seven years with religious solemnity, that disease has been known for thousands of years, and probably long before it was heard of in Egypt. In China, inoculation, which is there performed by thrusting a bit of cotton, imbued with matter, into one of the nostrils, has been practised from time immemorial.

# BOOK II.

## ORIGIN OF THE DOCTRINE OF CONTAGION.

## CHAP. III.

Those giant wits in happier ages born,
When arts and arms did Greece and Rome adorn,
Knew no such system.

RELIGIO LAICI.

*The doctrine that epidemic diseases depend upon contagion was unknown to Hippocrates, and the ancient Physicians —has not been entertained by any Pagan, Mahommedan, or Hindú nation; nor by Christians, until about the middle of the 16th century.—An Ordinance of the Police of Paris, respecting the Plague, in 1533, and Dr. Moulton's book, written about the same period, in English, are silent concerning contagion—it is not mentioned in any work written before, although it is spoken of in all works written after the translation of the Council of Trent to Bologna —Borde's Breviarie of Health—inconsistencies into which Lord Bacon, and Gibbon, have fallen, upon this subject—language of Shakespeare—testimony of Mercurialis, Sanctorius, and Septalius.*

EPIDEMIC and Pestilential Diseases, upon an average of nations, were at least as prevalent, in

ancient, as they are in modern times. During, as well as after the age of Hippocrates, the plague is stated, by historians, to have committed frequent and great ravages, both in the territories of Greece and Rome, and in those of the barbarians, as these nations were, in their pride, wont to designate every other people: and there is reason to believe that the yellow fever, now of such frequent recurrence in America, the West Indies, and some parts of Europe, was not, by any means, unknown to that celebrated physician [1].

In the life of Hippocrates, by Soranus, it is stated that "when a plague invaded the country of the Barbarians, viz. the Illyrians and Pæonians, and the Kings of those countries begged of him to come to them, he sent the messengers home

[1] In the ninth section of his book on Crises, he says, that "in burning fevers, a yellowness *(icteros)* breaking out on the fifth day, and accompanied by hiccough, is a fatal sign." In his book "De Ratione victus in morbis acutis," he says, that, "In a bilious fever, *yellowness* coming on with shivering before the seventh day, terminates the fever; but if it come on abruptly (or unseasonably), without shivering, it is mortal." In the first section of the first book of his "Coan Prognostics," he enumerates black vomiting among a number of the most desperate symptoms. *Melaina chole,* black bile, *melaina emeton,* black vomit, and *melanon emeton,* the vomiting of black matter, are phrases to be met with in the works which pass under his name.

with only their labour for their pains, after having first got out of them *the general course of the winds* there. Reflecting upon this, and concluding that the distemper would come to Athens, he foretold what would happen, and took care of the cities and the students [1]."

After the example of the great father of physic, epidemic maladies were, in former times, uniformly attributed, by the medical faculty, to the state of the air: and even amongst all the various causes, natural or super-natural, true or false, to which they continued to be attributed, by all the nations of the earth, for nearly two thousand years afterwards, it does not appear that they were ever supposed capable of being propagated by means of a specific contagion. This doctrine was reserved to illustrate the sixteenth century.

That the hypothesis of contagion, as the cause of epidemic diseases, should never have entered into the popular superstitions of any Pagan, Mahommedan, or Hindû nation, will probably be deemed a surprising, and scarcely a credible circumstance, by those, who are not aware, that these opinions were not always entertained even in Christian communities. It was not until about the middle of the 16th century, that this doctrine, in the manner, and for the purposes which shall

[1] Clifton's Hippocrates. His Life from Soranus.

be presently specified, was first promulgated, by the highest spiritual authority acknowledged in Christendom; where it not only immediately became of course the creed of the people, but was adopted by the medical faculty, and even embraced, from inadvertence, by the greatest philosophers, and the most eminent historians.

These facts, hitherto unexplained, will serve to unravel much of the confusion, and to reconcile many of the inconsistencies, which have prevailed in medical reasoning, particularly in regard to epidemic diseases, for the last two hundred and seventy years.

From the fall of the Roman empire, to the revival of learning in Europe, medicine only participated in the general darkness, which overspread science: and in the interval between the invention of printing, and the middle of the sixteenth century, that ignorance merely remained stationary. But, at this period, events occurred, which made the world, in respect to epidemic diseases, retrograde into false knowledge. Out of these events incidentally arose the too well known doctrine of contagion, as the cause of this important class of maladies. That this was the precise æra of its promulgation and establishment, is rendered manifest by the following facts, as well as by those which are elsewhere stated, in proof of the

opinions of the ancient Physicians upon that subject.

The facts to which I allude are these :—that, whilst, in books written previous to the translation, on the plea of a contagious epidemic, of the Council of Trent to Bologna, I have not been able to trace any mention of such a cause of epidemic diseases; it is notorious, that, subsequent to that event, every work, which has treated of these maladies, has either formally discussed, or gratuitously assumed that doctrine.

By an ordinance of the police of Paris, dated the 16th of September, 1533, I find, that, although houses in which persons were affected with plague were directed to be marked with a red cross, and persons coming out of them to carry a white wand in their hand; yet the houses were not called *infected*; no apprehension appeared to be entertained of the disease being propagated from one person to another; nor was the word *contagion* once used. The title runs thus; "Ordonnance de la Police de la ville et fausse-bourgs de Paris pour obvier au dangier de la peste et des mestiers prohibez fair en la ditte ville le nombre des medecins, cirurgiens, barbiers, et autres gens ordonnez pour visiter et medicamenter les malades de peste de laquelle Dieu par sa saincte grace nous vuélle preserver Publie a Paris le 16 Sept. 1533."

A book printed in English, in black letter, without a date, and not paged, which I suppose to have been written towards the end of the fifteenth, or early in the sixteenth century, bears the following title :—"This is the Myrrour or Glasse of Helth necessary and needefull for every person to loke in, that will kepe their bodye from the sykenesse of the pestilence, and it sheweth how the planetes do rayne in every houre of the daye and nighte, with the natures and exposicions of the xii signes, devyded by the xii monethes of the yeare, and shewed the remedies for many divers infirmities and dyseases that hurteth the body of manne."

There is here no idea of contagion. On the contrary, we find, in the enumeration of the contents, the air distinctly announced as the cause of the malady : "The second part of this treatise is how thou shalt gouverne and preserve thyself from the malice of the *pestilente ayre.*"

The author, as appears from the body of the work, is "Thomas Moulton, Doctour of Divinitie, of the order of the frere preachers,"—a circumstance, which, together with the typography, seems to confirm my conjecture, that this book must have been written whilst the art of printing was in its infancy, and whilst divinity and physic were yet united in the same hands; i. e. at any rate some time before the translation of the Council of Trent.

A person conversant in the history of printing might be able to infer the precise period of its publication. But such minute accuracy is not necessary to my object. I may however mention, for the satisfaction of those who are more curious, that the printer of one of the editions, for there appear to have been several, is stated, in the imprint, which is at the end, to be "Wyllyam Middelton at the sygne of the George in flete-strete, dwellynge next to Saynte Dunstones Churche."

With this exception, if it may be considered one, no medical writings of original character had yet appeared in England.

Linacre, who flourished at this period, was but a translator from the writings of the Italians.

Borde's Breviarie of Health, published in 1547, the very year of the Translation of the Council of Trent, is supposed, by Fuller, to be the first medical piece, written in English. It has what the author calls a prologue, addressed to Physicians, which begins thus: "Egregious Doctors and Masters of the eximious and arcane Science of Physick, of your urbanity, exasperate not yourselves against me, for making this little volume [1]."

That Moulton's book, however, must have been written earlier, I conclude, both for the reasons already stated, and because, treating of pestilence,

[1] Aikin's Biographical Memoirs, p. 55.

had it been written later, it would necessarily have adverted to the doctrine of contagion.

In the perusal of works, which have treated of epidemic maladies, it is of the greatest importance, with a view to a right understanding of the subject, to advert to the precise periods, at which they have been respectively written. Concerning their cause, it will be found that the opinions entertained previous, and those promulgated subsequent to the translation of the Council of Trent, shortly after the death of Henry VIII, essentially differ: and, from the events of that period, which, commencing with the year 1547, might, in respect to medicine, not unaptly be denominated the æra of contagion, I shall endeavour satisfactorily to account for these differences.

Until then, epidemic maladies had been usually denominated in England simply "Plague," and in France "Peste." From that æra, however, the very language employed in treating of them, appears to have undergone an entire change, extending even to the title of almost every book, or regulation, published upon the subject. Thus, instead of "Historia *Pestis* Avenionensis," which the title would formerly have probably stood, we have "Historia Avenionensis *Contagionis*," Avenione, 1633. Before this language had prevailed quite half a century, the recollection seems to have been obliterated in Christendom, that opi-

nions of a different, or opposite nature, had ever existed.

Hence, from inconsciousness that any change of doctrine had taken place at this period, so many eminent writers, of more recent date, have naturally fallen into error, not only in point of reasoning, but even in point of fact. From this number, it will be sufficient for my present purpose to select two of the most splendid instances, which could any where be found, in those distinguished ornaments of science and of literature, the great Lord Bacon, and the illustrious historian of the decline and fall of the Roman empire.

Speaking of a fever, which ravaged England in the reign of Henry VII, Lord Bacon says: "It was conceived *not* to be an *epidemic* disease, but to proceed from *a malignity in the constitution of the air*, gathered by the pre-dispositions of the seasons[1]." In the former part of this sentence, the term epidemic, in conformity with the new language of the period subsequent to the Council of Trent, is obviously used as synonimous with a disease depending upon contagion; whilst, in the latter, the malady is properly attributed, according to the language of the period preceding that Council, and of the time at which

[1] Henry VII.

the pestilence in question occurred, to the state of the atmosphere. This palpable confusion of ideas is now easily accounted for. But here, I cannot help figuring to myself, what would be the surprise of Hippocrates, if he could rise from the grave, and be told that a disease, proceeding from the malignity of the air, is *not* an epidemic; or that epidemic diseases depend upon a cause, which he had never known, or heard of, in fevers, called *contagion!*

The errors thus promulgated, by authority, in the 16th century, have also served to throw a false colouring over the history of epidemic diseases, even in antecedent times; and to mislead persons of the greatest discernment, respecting them. In the interesting and original picture of the manners of Rome, quoted by Gibbon, from Ammianus Marcellinus, we find, for instance, the following remarks: "It is allowed as a salutary maxim, that the light and frivolous suspicion of a *contagious* malady is of sufficient weight to excuse the visits of the most intimate friends: and even the servants, who are dispatched to make the decent enquiries, are not suffered to return home, till they have undergone the ceremony of a previous ablution [1]."

[1] The History of the Decline and Fall of the Roman Empire, edit. of 1807, vol. v. p. 276.

That, by a contagious malady is here meant a fever, or epidemic disease, supposed to be capable of propagating itself from person to person, if there could be any doubt upon the subject, would be confirmed by what is immediately afterwards stated, in relating the events of the first siege of Rome, by the Goths, under Alaric, in the year 408: "And the miseries of famine were succeeded and aggravated by the *contagion* of a *pestilential* disease [1]."

In perusing these statements, it has appeared to me that the elegant historian of the Roman empire had been inconsciously applying the doctrines of the 16th to the events of the 5th century; since the text of Ammianus Marcellinus, written so long before these doctrines were in existence, could not be supposed, unless it were in the spirit of prophecy, to have any reference to them. Accordingly, we find, upon examination, that, amongst the liberties, which he professes to have, upon this occasion, taken with his original, Gibbon acknowledges to "have developed some observations, which were *insinuated*, rather than *expressed* [2]; and, although it may easily be conceived how he might,

[1] The History of the Decline and Fall of the Roman Empire, vol. v. p. 292.

[2] Ibid. p. 266, note.

with perfect sincerity, have fallen into this error, or rather how difficult it was, in a free translation, to have avoided it, considering the state of existing opinions upon this subject, in which the historian cannot be supposed not to have participated; yet it cannot be admitted, that any thing in the text of Ammianus Marcellinus, amounts to an insinuation of the nature here made, or is a sufficient foundation for the observations quoted.

This much I have thought proper to state, in order to obviate cavil, although it may be only unnecessarily anticipating objections. But, with a view to ascertain more precisely the state of the case, I have examined and compared several editions of Ammianus Marcellinus, in the original; especially one of 1474, and the Leyden edition of Gronovius, of 1693; without being able to meet with any expression signifying what is usually understood to mean contagion, as applied to epidemic diseases. The following is the only part, upon which I can suppose Gibbon's assumption to have been founded. "Et quoniam apud eos, ut in capite mundi, morborum acerbitates celsius dominantur, ad quos vel sedandos omnis professio medendi torpescit; excogitatum est adminiculum sospitale, ne quis amicum perferentem similia videat: additumque est cautionibus paucis remedium aliud satis vali-

dum; ut famulos percontatum missos, quemadmodum valeant noti hac ægritudine conligati, non antè recipiant domum, quam lavacro purgaverint corpus. Ita etiam alienis oculis visa metuitur *labes*[1]."

Along with the original, I think it right to give the translation, by Dr. Holland, of the above passage, in 1609, sixty years after the doctrine of contagion was promulgated, by which means every reader will be able to judge for himself, how far I am correct in the opinion that the translation has been warped in conformity to that doctrine. "And for as much as among them, as in the head place of the world, grievous diseases reigne and rage in the highest degree, for the allaying and assuaging only whereof, no art of physicke is of any force; devised there is a preservative and helpefull meane therefore, to wit, that no man should visite and see a friend, that suffereth such maladies. And among those few warie cautions and provisoes there is another effectuall remedie ordained, namely, that such servants as have been sent to enquire how those of their acquaintance do that are held with this disease, they receive not into the house again, before they have cleansed their bodies with a bathe: so fearfull be they even of an *infection*, that other men's eyes only have seen." (P. 13.)

[1] Ammian. Marcel. lib. xiv. cap. vi. edit. Gronov. Lugd. 1693, p. 24.

There is some slight variation between this and the edition of 1474, but not materially to affect the present subject.

Of this passage it is to be remarked, that, although it states grievous diseases to have raged at Rome, for the alleviation of which the art of physic was of no avail, no disease is mentioned by name; a reserve which could not have been thought necessary, if the observations had related to fevers, or epidemic diseases, respecting which mystery would have been wholly misplaced. The reserve, however, was proper and decent in an historian, if the observations be supposed to refer, as was most probably the case, to syphilis, or some other malady reputed scandalous, which might have been known, although not common in those days.

These remarks, as well as several allusions, which are to be found in the latin poets, if such enquiry were not foreign to my present subject, might perhaps serve incidentally to throw some new light on the history of lues venerea. But whether the paragraph quoted from Ammianus Marcellinus be supposed to relate to that, or to some other disorder reputed scandalous, it is most certain that it cannot, in any manner, be construed to refer to epidemic diseases, to which, at that period, neither terror, nor odium, nor scandal had been attached. I am not aware

that previous to the 16th century, any precautions had ever been taken against the propagation of fevers, from person to person, which would certainly have happened, had the idea of contagion, as connected with them, been earlier entertained.

It is to be remarked, that, in Dr. Holland's translation of Ammianus Marcellinus, "*labes*" is improperly rendered "*infection*," according to the erroneous meaning now affixed to that word. Literally "*labes*" signifies a spot, or stain; and, metaphorically, contamination, as "communis vitii *labe* corrumpi." But, in this case, we must infer the mark to have been distinct and unequivocal, since it was stated to have been, "*alienis oculis visa.*"

The text of the Roman historian was printed previous to the æra of contagion, and could not be altered in compliance with that doctrine, whilst the English translation, was almost necessarily made to correspond with the doctrines upon the subject, which were then universally received.

When men like Lord Bacon and Gibbon could have been led, by any chain of circumstances, into so stupendous an error, it cannot be deemed surprising that, in Christendom, it should have been universally received. Nor ought it to mortify the self-love of any man, however great his talents, or exalted his reputation, to find himself, upon a subject which it is

not his immediate province to investigate, misled in such illustrious company.

It is curious to observe how Shakespeare, who flourished about half a century after the translation of the Council of Trent, blends the phraseology of the doctrine of contagion with the language of nature :

> ————Plagues, incident to men,
> Your potent and *infectious* fevers heap
> On *Athens.*————
> *Timon of Athens,* Act iv. Scene 1.

> As wicked dew, as e'er my mother brushed
> With raven's feather from unwholesome fen,
> Drop on you both! *Tempest.*

> All the *infections* that the sun sucks up
> From bogs, fens, flats, on Prosper fall, and make him
> By inch-meal a disease! *Ibid.*

The first of these wishes involves the modern doctrine of contagion unmixed; the second the doctrine of nature unsophisticated; and the third a combination of both. "Infectious" and "Contagious" are here evidently used as synonimous terms; the former certainly, and the latter most probably, in a sense very different from that, in which they were employed by the ancients. With them, *infectus*, signified literally, dyed, or stained; metaphorically, poisoned, envenomed, vitiated, corrupted. Infected by poison—infected

by the pestilent air—did not infer, that the diseases, produced by these agents, could be communicated, by means of contact of persons labouring under them, with persons in health. If I mistake not, the idea of contagion, as connected with plague, or fevers, or epidemic diseases of any description, is not to be met with previous to the sixteenth century: and if the ancients are ever made to use a language, which may bear that construction, it is, I am persuaded, entirely owing to the industry, or mistakes, of physicians, commentators, dictionary-makers, or interpolators, of that period, or their successors. At least I must remain of this opinion, until I see some proof of its being erroneous. Peste *afflari; affici; laborare;*—to be *scorched with;* to be *affected with;* to *labour under,* the plague.—were terms commonly used by the antients. And when we find the English word "*infectious,*" translated into Latin, in the Dictionaries, "*pestiferus,*" "*morbidus,*" "*nocens,*" it can scarcely fail to appear a most arbitrary and unwarrantable interpretation, of a word itself employed in an improper sense. Amongst the ancients, even the terms "*contagio*" and "*contagium,*" were, for the most part, used metaphorically, to signify contamination, corruption, defilement, &c. as the "lucri contagia" of Horace. If we even admit that the "pecoris contagia" of Virgil, was

meant literally to express a disease capable of being communicated by one living body to another, it will still require more evidence than I am at present aware is in existence, to shew, that, the epidemic diseases of men have ever been considered, by any physician, poet, or historian, or even by any of the vulgar, amongst the ancients, as capable of being propagated by contagion.

That the doctrine of contagion was unknown to the ancients, is farther confirmed by the testimony of several authors, who wrote about half a century after it was first promulgated. Amongst others, Hieronymus Mercurialis, who taught in more universities in Italy than any one, and with greater encouragement from princes, makes this positive declaration: "After I had made a most particular search among the writings of ancient physicians, for the plague being communicated by a fomes, I never could discover any such opinion amongst them: but, what is very remarkable, no modern physician, who has either taught physic, or has translated the works of the ancients, ever offered at an explanation, of a matrice, or fomes, till the times of our grandfathers [1]."

In 1614, Sanctorius says that "we are in-

[1] Alluding, as will presently appear, to Pope Paul III, his Legates, and physician Fracastorius.

fected with the plague, not by the touch, but by drawing in the pestilential air with our breath."

Ludovicus Septalius, an eminent physician of Milan, in his book, "De Peste et Pestiferis Morbis," published in 1622, also affirms that the manner of conveying infection, by a fomes, was unknown to the ancients, and never thought of by them.

It may be observed, that, Homer, when he mentions the dreadful plague, which he says Apollo sent among the Grecian army, because Agamemnon, their general, had taken and detained Chrises' daughter, does not speak of contagion, as the means by which it was propagated; an omission, which, in respect to so important an agency, if it had been known, would have been unpardonable, or rather indeed impossible, in any poet. On this occasion, in whatever degree the story of the siege of Troy may be supposed to be founded in reality, it does not appear unreasonable to infer, that the great author of the Iliad, uniting the philosopher with the poet, might, in these lines, have contemplated the ravages of a pestilence, which he knew might be occasioned, in an army long encamped upon a sandy plain, and exposed to the influence of a scorching sun; choosing poetically to represent the intense rays of that luminary, as the anger of a god:

Th' insulted sire (his god's peculiar care)
To Phœbus prayed, and Phœbus heard the pray'r.
A dreadful plague ensues, the avenging darts,
Incessant fly, and pierce the Grecian hearts.

POPE'S HOMER.

No man is bound to prove a negative. And, if, after the circumstantial evidence which has been adduced, any one should still maintain that this opinion did exist, amongst physicians, previous to the period mentioned, the onus will lay upon him of shewing the grounds of his belief. It will be incumbent upon him to point out what ancient physician did entertain; upon what occasion, and to what degree, he entertained; and in what manner he supported that doctrine.

So much as to the æra, of the promulgation of the hypothesis of contagion. Its truth or falsehood is not at present in question. If it were true, it would follow that in imputing epidemic diseases to the air, the ancient physicians had been labouring under a gross delusion, for 2000 years; or if it be false, that in imputing them to contagion, the modern physicians have been labouring under a delusion still more extraordinary, for 270 years. From this dilemma, there is no escaping, but by forming a third conclusion, more extravagant than either, viz. that, in ancient nations, pestilential diseases depended upon the air; but, that, in modern states, they depend upon contagion!

## CHAP. IV.

Thus in a pageant show a plot is made.
ABSALOM AND ACHITOPHEL.

*Inconsistency of the belief in contagion, as the cause of epidemic diseases—the doctrine invented in the year* 1547, *by Pope Paul III, to frighten the fathers of the Council of Trent, and to serve as a pretext for translating that Council to Bologna—a book of medical tales published by Fracastorius, physician to the Council, in* 1546, *to prepare the public mind for the stratagem—is the first physician, who avows this opinion—which he supports upon absurd, and evidently false grounds, notoriously invented for the purpose.*

IN tracing the origin, and progress, of the doctrine of contagion, in epidemic diseases, what must strike the unbiassed observer with the greatest surprise, is, that, it should ever have happened to any man, to conceive so extraordinary an idea as, that it could be consistent with the laws, by which the Author of nature governs the universe, that the human species should be liable to be affected with diseases, of

a most destructive kind, capable of being propagated by contagion, and of affecting the same person repeatedly; *i. e.* incapable of ceasing, where no precautions are employed, but with the extinction of communities: and, if any thing could add to his surprise, it would be to find, that, the person, with whom such an idea originated, was no other than the head of the Christian church.

Having, however, ascertained the fact, he would farther marvel, how it was possible that the inventor of this doctrine, if he could allow himself to consider the cruel inconsistencies, which it involves, as compatible with the attributes of divine wisdom, and mercy, should not have been led to hesitate in his belief, upon perceiving, that its inevitable consequences did not follow, in the extinction of communities. The successor of St. Peter, it may fairly be presumed, would not have entirely overlooked these inconsistencies, if he had not had a grand political purpose to serve.

When, at the court of Rome, it had become proverbial, that no revenue could be too much for a cardinal, unless it were more than enough for a king; when the sale of indulgences, the abuse of pluralities, and other corrupt practices of that court, together with its alarming encroachments upon the temporal authority of

princes, had occasioned those multiplied scandals, which helped to provoke the reformation; a general council, or synod, was called for, by the princes, as well as by the people of Christendom, as the most likely means of healing religious dissentions [1].

[1] Under Leo X. indulgences and pardons for sins, were publicly sold throughout Christendom, extending them even to the dead. The revenues of diverse provinces were bestowed, by his Holiness, upon diverse persons. The indulgences of Saxony, he gave to his sister, Magdalen, wife of Franceschetto Cibo, a bastard son of Pope Innocentius VIII. She, in order to turn the privilege to the best account, employed Bishop Aremboldus, to publish them. The inferior pardon-mongers, in the service of Aremboldus, employed the Dominican friars, at a reduced agency, to retail them to the people. This measure gave great offence to the Heremite friars, who had, till then, enjoyed that privilege, and of whom Martin Luther was a member. The rest is known. *History of the Council of Trent, by Pietro Soave Polano, translated into English, by Nathaniel Brent, folio,* 1620, *pp.* 4, 5. It may be proper here to state, that, such was the title, under which, to avoid papal persecution, the justly celebrated history of the Council of Trent, by Fra. Paolo Sarpio, theologian of the most serene Republic of Venice, was originally published. It is the translation of Brent, from which my quotations are taken, and to which my references are made. I have compared this with a French translation, by the Sieur De la Mothe Josseval, secretary to the French embassy at *Venice*, printed at Amsterdam, in 1683, from an edition, published in the name of the author; and found them to correspond. I have chosen to adopt

The Popes, for a long time, and under various pretences, declined, or evaded, the convocation of a council, dreading that the proceedings of such an assembly, if they could not be sufficiently restrained, might tend to weaken their authority, or to revive the question, respecting the superiority of a general council, over the Popes. They were, however, obliged, at last, to yield to the necessities of the times; and the celebrated assembly, known by the name of the holy œcumenical Council of Trent, convoked, according to the Pope's Bull, "to heal the wounds of the Church, caused by impious heretics," held its first session, in that city, on the 13th of December, 1545 [1].

Brent's translation, in preference to giving a new one, since, as marking the stile and character of the age, with scarcely any of its obscurity, it may prove more acceptable to the curious reader.

[1] *History of the Council of Trent, p.* 130. It is matter of surprise, that, since the council was considered as the best, or only, remedy, for the numerous existing evils, the Popes were able to evade assembling it so long, especially when we reflect that "abuses had attained to such an incredible degree of enormity," that, "in the year 1534, Clement VII. was not ashamed to commend unto his nephew, Hippolitus, Cardinal de Medicis, *all the benefices of the world*, secular, and regular, dignities, and personages, simple, and with cure, being vacant, for sixe moneths, to beginne from the first day of his possession, with power to dispose of, and convert to his use, all the fruits," &c. *History of the Council of Trent, p.* 251.

Paul III. during whose papacy this synod first assembled, (pending its sitting, and the treaties concerning it, there reigned, and died, eight Popes), although, in compliance with the humour of Germany, which it would not have been prudent openly to resist, he had agreed to convoke the Council at Trent, had, however, predetermined to translate it, in due season, to a place, where his authority was more absolute. This we learn from the history of the times, as well as from the date of the Bull, authorising that measure, viz. the 22d of February, 1545, and the period of its transmission to the Legates, in August of the same year, being four months previous to the holding of the first session. In this measure, the Pope resolved to keep himself out of view, and to let it appear to be the act of the Legates. If it should be opposed, they would be blamed, and he, as not being a party, might the more easily uphold them: and if, by chance, he should alter his opinion, he might do so, without dishonour [1].

In 1547, it being doubtful whether the Pope had most to apprehend, from the progress of the Reformation, the policy of the Emperor, or the decisions of the Council, if left free, it was finally resolved to carry the measure of translation into

[1] History of the Council of Trent, p. 259.

effect. A private gentleman, of the family of Cardinal Monte, was sent to the Legates, with orders not to arrive at Trent before the opening of the session, and then to give them authority to translate the Council to Bologna, "*making some apparent cause to arise,* or *making use of some cause already in being;* putting it in execution so quickly, that, after the enterprise begun, they should conclude, before any impediment could be interposed[1]."

On the 3d of March, the Pope's messenger, until then concealed even from the Legates, declared his credence[2]. I will give the "*cause, which was made to arise,*" or the pretext for the translation of the Council, in the words of my authority: "The Cardinal Santa Croce[3] was confounded; but Monte[3], being without feare, said, that he ever knew the Pope to be a wise Prince, and that he then saw in him the height of human judgement, that it was necessary to do so, if they would preserve the see Apostolike: and therefore that it was fit to serve his Holinesse with fidelity, secrecy, and diligence. And it hapned fitly, that many in the families of the Prelats were sicke, either by the disorders of the

[1] History of the Council of Trent, p. 259.

[2] Ibid. p. 265.

[3] Two of the Legates; Cardinal Poole, the third, being absent. *History of the Council of Trent, p.* 266.

Carnoval, or because the air had been moist many days before. Monte suborned some to aske the physitians whether those infirmities were not contagious. The physitians, who, in their prognostiques, always say the worst they can, because, if they proove true, they seeme learned, in having foreseene them, if not, they seeme more learned, in remedying or preventing them, spake some ambiguous word, which was studiously spread abroad, and believed by some of the meaner sort. Some others also were credulous of it, especially those who were willing to depart, and therefore wished it were true. And, after the session, a Bishop dying opportunely, interred with the obsequies of the whole Council, made the matter more conspicuous. Whereupon all Trent was full that the disease was contagious, and the fame was spread in all the bordering places. The Legats, to shew that they had no finger in dispersing this fame, held a general congregation, the next day after the session, to digest what should be discussed concerning the Eucharist, and, the weeke following, the congregations of the divines did begin. But the fame being increased, Monte gave order to Hercules Severolo, Proctor of the Council, to make a proces concerning the pestilent infirmity. The physitians were examined, and Jeronymus Fracastorius, amongst others, who had the title of

Physitian of the Councel, and other persons besides. It was reported that the neighbour places would have no more commerce with the citie; which made many Prelats aske leave to depart, either for feare, or for desire to go from thence by all means. Monte gave leave to some, that their departure might be alledged as one of the causes; others, with whom he was more familiar, he perswaded to tarry, indeed, not to want adherents when he should propose the translation of the Councel, but, in shew, that the Councel might not dissolve; and therefore he wished them to require in the congregations, that some order might be taken. The proces was prosecuted until the eighth day; when newes came, either true or fained, that Verona would traffique no more with them: which troubled them all, because they should be as it were all in prison.

" Therefore the 9th day, a generall congregation was held concerning this: in which the proces was read, and a proposition made, what remedy might be found that they might not be confined, with a disease in their houses, and deprived of victuals and other necessaries. Many protested they would depart, and could not be held, and, when much had bin spoken, Monte proposed the translation of the Councel, saying he had Apostolike authority to doe it from the be-

ginning; and caused the Pope's Bull to be read, directed to the three Legats, Monte, Santa Croce, and Poole. In which, having declared that he had established the Councell in Trent, and sent them thither as his Legats, and Angels of Peace, to the ende that so godly a worke might not bee hindered by the incommoditie of the place, he giveth power to any two of them, in absence of the third, to translate it into some other citie, more commodious, opportune, and secure; and commend the Prelats, upon censures and punishments, not to proceed any farther in Trent, but to continue it in the citie, unto which they shall transferre it, and to call thither the Prelats, and other persons of the Councel of Trent, upon pain of perjurie, and other censures, set down in the letters of the convocation, and that he will ratifie whatsoever they shall doe, any thing to the contrary notwithstanding.

"The Emperor's prelats answered immediately that the disease, and the dangers were not so great; that the timorous might have leave to goe, until the opinion were past, which, by the help of God, would vanish quickly, and, if the session were deferred, it was no matter: that many departed the yeere before, for the suspicions of warre, and the session was deferred more than sixe months, and that they might doe so now, if there were occasion. And other such

reasons they brought. The disputation concerning this was long. When the congregation was ended, the Imperialists, conferring among themselves, and seeking curiously that, which, before, they cared not to know, they smelt out that it was not a generall disease, but a pretence [1]."

[1] *History of the Council of Trent, &c. pp.* 266, 267. In the able preface to the French translation, I find some farther light thrown upon the circumstances, which attended the translation of the Council of Trent, to Bologna. We may infer that the sensation was not trifling, which this history occasioned, since, in a counter history of that Council, which the Cardinal Palavicini thought fit to issue, he calls that of Fra. Paolo, originally published, as we have seen, under the name of Soave, "*Scelerata Istoria*", and "*Istoria impiamente famosa.*" In chapter 13th of his ninth book, he endeavours to persuade the reader that the translation of the Council was made, on account of contagion, and that the disease was *scarlet fever ;*" that the Bishop of Capaccio, and the general of the Cordeliers, died of it ; and that, Jerome (Hyeronimus) Fracastorius, physician to the Council, protested, that, as he was come to cure fevers, and other ordinary diseases, but *not* the *plague*, he would withdraw : "Voila des preuves bien legeres," says Mr. de la Mothe Josseval. Le Cardinal dit que cet maladie etoit le pourpre. Aujourd'hui le pourpre, *quoiqu' il se gagne* [1], ne fait point enfuir les Medicins : Et l' on aura de la peine à croire, que Fracastor en eût tant de peur, s'il etoit aussi

[1] The French translator here participates in the common error, which had become general long before his day.

The next day, at a congregation, called to consult upon the same matter, it was found that eleven prelates had already departed; and the legates, after some conversation, proposed that the Council should be translated to Bologna.

habile, que le Cardinal nous le figure, comme je n'en doute point. Et s'il demanda son congé, c'étoit un jeu fait a plaisir entre les legats et lui, *pour efraier les Peres.* La mort de l'Evêque et du General ne conclut rien. Car ce n'est pas merveille, que d'un grand nombre de gens, assemblés dans un même lieu, ou l'on n'a pas toutes ses commodités, il en meure deux personnes. Les Legats pouvoient perméttre de se retirer a ceux, qui craignoient leur peau, d'autant que le mal n'etoit pas si grand, qu'il fut besoin de transferer le concile, ainsi que les Evêques Espagnols le remontroient. En éfet, il ne leur arriva point de mal, durant les deux ans, qu'ils restérent à Trente, quoique Fracastor, et le Medicin du premier Légat eussent prognostiqué, que le danger etoit grand, *surtout pour les gens nobles.* Comme si la noblesse rendoit les hommes plus susceptibles de maladie. Chose ridicule. Car le Cardinal Paceco, l'Evêque de Salamanque, Frere du Duc de l'Infantado, l'Eveque d'Astorgas, et d'autres, etoient d'asses bonnes maisons, pour en mourir. Onufre Panvini, qui vivoit, de ce tems là, à la cour de Rome, et en savoit tres bien la carte, ne dit rien de cette contagion, dans la vie de Paul III, ou il parle de la translation du concile." I may observe, in addition, that the deaths, among the members of the Council, during its sitting, or at any of its periods, were probably fewer, than would have happened, amongst the same number of persons, if they had been dispersed, and at their several homes.

It being suggested that the Pope might take it ill, if the translation were made without his knowledge [1], Monte said, "that sudden chances, and perils of life, are free from these respects, and that hee would undertake the Pope should be contented. The decree was proposed by way of deliberation—Doth it please you to declare, that this disease is manifest, in regard of the predictions, and other things alleadged so notoriously, that the prelats cannot remaine in this citie, without danger of their life, nor can be kept here against their wills; and because of the departure of many, and protestations of others, by whose departure the Council would be dissolved; and for other causes alleadged by the fathers, notoriously true and lawful? Doth it please you to declare, that, for the securitie of the prelats lives, and prosecution of the Councel, it ought to be transferred to Bolonia, and is now transferred, &c [2]."

On the following day, the 11th of March, at the regular session, the decree for the translation was formally passed, by a majority of the prelates; and his own measures were of course sanctioned by the Pope [3].

[1] This must have referred merely to the time and place of translation, since the power to translate generally, had been conferred by the bull previously read.

[2] History of the Council of Trent, &c. p. 267.

[3] Ibid. p. 268.

But "the imperialists were commanded, by the Emperor's ambassador, not to depart, untill his Majesty were informed, and gave them order[1]."

"In Rome, the Court was glad they were delivered from danger. For, there was already great confusion, and sale made by the possessors of many benefices, who sought to unburthen themselves, but *so as that they would lose no commoditie*[2]."

The Emperor is offended, and sends ambassadors to Bologna, who protest, among other things: "that the Councell could not be translated, but by urgent necessitie, diligent discussion, and consent of all; but, for all this, they, who call themselves legates, and others, did rashly runne out of Trent, fayning certaine fevers, and infections of the ayre, and testimonies of doctours, which the event hath shewed to be causes not so much as of any vaine feare[3]."

His ambassador, at Rome, made a similar protest, declaring, in express terms, that the pretences, upon which the Council was transferred, were "neither true, nor probable[4]."

The Spanish prelates, who had remained in Trent, state, in answer to a citation of the Pope:

[1] History of the Council of Trent, &c. p. 268.
[2] Ibid. Id. p. 280. [4] Id. p. 281.

" that they saw not why they should depart with the legats, who promised, in the generall congregation, and in the publicke sessions to returne to Trent, so soone as the suspition of the sicknesse did cease, especially if Germanie would submit to the Councell ;" and that Germany had submitted [1].

This answer, the Pope, affecting not to be a party, but umpire, or judge, sends to the proctors, of the fathers of Bologna, who reply, " that it was necessary to dissolve or translate the Councell, in regard to the progresse which the pestilential sicknesse made in the citie, and borders, of the actuall and imminent departure of many fathers, of the doctour's oath, especially of *Fracastorius who had a publike stipende,* and of the feare that the commerce of the neighbour cities would be taken away."

We have now seen, that there existed no sickness at Trent, beyond the usual diseases of the season ; that the Spanish prelates were not afraid to remain, and that they remained with impunity, after those of Italy had gone to Bologna ; and that the latter did not think of returning to Trent, even after every idea of a contagious malady had long ceased. Hence it became obvious to all the world, that the ground, set forth,

[1] History of the Council of Trent, &c. p. 285.

for the translation of the Council, was nothing more than a mere pretence, whilst the real object of the measure was well understood to be, the having that assembly in a situation more immediately within the influence of the court of Rome.

The reader will be pleased to advert to the use that was made of Fracastorius, upon this occasion. It has already been shewn that the Pope had predetermined to translate the Council; that his legates were directed "to make some apparent cause to arise," which might justify this measure; that the cause, which the legates "made to arise," was a *contagious pestilence*; and that Fracastorius, physician to the Council, *with a stipend*, was directed to swear to the existence of this strange malady. It is also necessary that the reader should be informed of another link, in this chain of conspiration, which does not appear in the History of the Council of Trent; viz. that, in the interval, between the date of the Pope's bull, and the actual translation of the Council, the public mind was duly prepared for the new phenomenon of *contagious epidemics*, by this very Fracastorius, in a work, entitled "*De Sympathia et Antipathia, et de contagione et contagiosis morbis, Venetiis*, 1546." He will then perceive that Fracastorius was a mere creature of the Pope, and of the

Pope's Council; and he will have a distinct view of the whole of this ingenious, but very mischievous, plot.

To the doctrine of contagion, in fevers, thus fraudulently, and for the first time authoritatively introduced, into the world, may be imputed the annual destruction, as shall be fully shewn, of many millions of our fellow-creatures, and the misery of many millions more.

But physicians of any eminence, excepting such as were willing to be deceived, were not the dupes of this imposture. Nor is it credible that Fracastorius himself, who was undoubtedly a man of wit and learning, could have had any faith in his own doctrines, supported as they were, only by the most monstrous absurdities [1].

[1] Fracastorius was an eminent physician, and a person of great learning, as well as a poet, of a lively imagination. Beside his book, "*De Sympathia, et Antipathia, et de Contagione, et Contagiosis Morbis,*" he was the author of a poem, entitled, "*Syphilis;*" which shews at least that he was conversant with the subject of contagion, where it is acknowledged to exist. *Friend's History of Physic*, vol. ii. p. 369; *and Sir Richard Manningham's Discourse, concerning the Plague, and Pestilential Fevers, London*, 1758, p. 32. Sir Richard says, that Fracastorius was the first, who made any change in the ancient and common opinion. But, if my view of the subject be correct, the physician only obeyed orders, and that distinction more properly belongs to Pope Paul III.

" Out of one leather *coat*," for instance, (another edition has it a leather *cap*) " there died," says he, " five-and-twenty Germans, who put it on one after another." This vague assertion of an alledged fact, said to have happened, thirty-five years before, during a plague, at Verona, which even the narrator does not pretend to have witnessed, and of which he gives no testimony on the authority of others, is most presumptuously offered, in proof of a doctrine of the most essential importance to mankind. Did we not know, upon general principles, that contagion does not exist in epidemic diseases, we should be bound in reason to discredit this narrative, not simply from the want of all evidence, but from the very suspicious circumstances of the scene being laid in *Italy*, and the victims being all *Germans*.

But to reason against absurdities, is to fight against shadows. And I have only here adverted to this silly tradition, as being the first formal attempt, which was made, in this way, and unfortunately a most successful one, to impose upon the understandings of mankind; and in order that it may appear, that, the necessity of having recourse to an expedient so ludicrous, in support of a doctrine of such singular importance, is, at once, a proof of its want of foundation, and of a deliberate intention to de-

ceive. This leathern *garment* (for so it has also been termed [1]) is a morsel, which no ordinary effort of faith can digest. If the fact could, for a moment, be credited, the interpretation of it must be, that the cloak, coat, garment, or cap, contained *poison*, not plague.

The following traditions are of a similar description: and although not necessary to be reasoned against, they are proper to be recorded, as being intimately connected with the singular events of this extraordinary æra, and constituting the whole foundation of the stupendous error of contagion.

Alexander Benedictus, Lib. de Peste, cap. 3, tells us of a "feather bed, that was thrown aside, into a remote corner of the house, being suspected to hold the plague in it; but that it raised the plague, by being shook up, *seven* years after, of which 5900 people died, in twelve weekes, in Wratislaw. And we are told that the pestilent contagion was shut up in a rag, for *fourteen* years!"

Forestus, Lib. vi. Obser. 22, says, "that a young man was seized with the plague, only by thrusting his hand into an old trunk, wherein there was a *cobweb*, which, in that instant, made a plague sore."

*These three* are the principal alledged facts,

[1] City Remembrancer, vol. i. pp. 154, and 257.

brought by Sennertus, to prove that plague is a *contagious* disease, and seem to be the only foundations for *quarantine*[1].

With these tales, may rank the following one of recent date. A brother of the French general Julien died of the plague in Egypt. "He received the infection by taking a pinch of snuff, from a box, out of which a person, who had the plague on him at the time, had also taken snuff[2]."

I met, at Constantinople, with a statement equally singular of a supposed exception to the capability of receiving infection: "Of all quadrupeds, the shaggy horse, or horse with long hair, is *alone* exempt from contracting the infection of plague. Other animals, and birds of every kind, can receive, and communicate the infection[3]."

[1] Sir R. Manningham's Discourse, p. 25.

[2] Travels in Turkey, Asia Minor, Syria, &c. in 1799, 1800, and 1801, by W. Wittman, M. D. p. 76.

[3] From an unpublished manuscript, put into my hands, by Mr. Julius Cæsar Kelli, a native of Leghorn, containing Observations made during the Plague at Salonica, in 1783, and at Brusa, in 1800, 1812, and 1813. Into the grounds of these assertions, I did not think it necessary to enquire.

## CHAP. V.

Immortal lies on ages are entailed.
RELIGIO LAICI.

*The reception of the doctrine of contagion, in epidemic diseases, facilitated by the progress of small-pox, and lues venerea, in Europe, in the 16th century—favoured by the still-existing union of the medical and clerical functions—by ignorance of the medical works of the Greeks—by the overwhelming influence of the see of Rome—by the danger to physicians of controverting it—introduced by policy, received upon credit, and propagated by faith, it has continued without examination, become fixed from habit, and occasioned a greater destruction of lives, than the ambition of all the conquerors, who have deluged the earth with blood, from Julius Cæsar, to Napoleon Bonaparte.*

To facilitate the reception and propagation of this delusive doctrine, although the influence of papal authority alone would probably have been sufficient, several circumstances, at this period, concurred.

By the spreading of small-pox and lues venerea, in Europe, the public mind had become gradually familiarised with ideas of contagion; whence,

the application of the doctrine, to epidemic diseases, however in truth absurd, might have appeared a less violent innovation, than it would have done a century or two earlier [1].

The union between the medical and clerical functions, which existed, in Europe,

> When want of learning kept the laymen low,
> And none but priests were authorized to know—

in full force, almost down to the period, of which we are treating, whereby the Popes enjoyed nearly as unlimited an authority, over physic, as they did over divinity, also greatly contributed to extend, to confirm, and to establish this new doctrine of contagion.

To give a few examples of this union amongst ourselves: John Giles, who flourished in the 13th century, was a professor of medicine, in the universities of Paris and Montpellier, and physician in ordinary to Philip, king of France. He was *afterwards* created a doctor of divinity, and was the first Englishman who entered among the Dominicans, with whom he became a celebrated

[1] In the course of the 14th century the small pox spread in Europe. In the 15th century lues venerea became generally known in this quarter of the world, and, towards its close, in the year 1494, spread greatly at the siege of Naples. *Friend's History of Physic, Vol. II. p.* 337.

preacher. In his old age, he was famous for his divinity lectures, at Oxford [1].

Hugh of Eversham, or Hugo Atratus, was, in the same century, both a physician, and a cardinal. He died of the *plague*, with several other cardinals, of the conclave, held after the death of Pope Honorius IV. Bale, who seldom allows a cardinal to die a natural death, says he was poisoned [2].

John Phreas, in the beginning of the 15th century, was created by Paul II. bishop of Bath and Wells, and practised physic in Italy [3].

Linacre, the founder of the London College of physicians, and one of the earliest English medical translators, took holy orders in 1519. He inscribed some of his translations to Pope Leo X [4].

Thus we see, that, in the early part of the 16th century, medicine was still in the hands of men, who were either ecclesiastics, or had received a monastic education. Many had taken holy orders, and some even held cures. Hence an intimate connection obtained between medicine and the see of Rome; a circumstance that will lessen our surprise, at finding even medical doctrines, when, for convenience, promulgated

[1] Aikin's Biographical Memoirs, Introd. pp. 5, 6.
[2] Ibid. p. v. [3] Id. p. 25. [4] Id. pp. 32, 40.

by papal authority, received as articles of faith, which it would be heresy to doubt.

Even in England, although the connection between the government and the see of Rome, was, for a time, suspended, by the humour of Henry VIII, yet the influence of papal doctrines continued still considerable; and, in medical matters especially, the tone was then, and continued long after to be received from Italy [1]. Hence the doctrine of contagion was implicitly embraced in England, and acted upon, with a vengeance, during the epidemics that prevailed, in the bigotted reigns of the Stuarts. The destruction, which they occasioned, was all imputed, by their enemies, with equal bigotry, to the crimes of that family.

It may be also observed, that the doctrine of contagion had the fewer obstacles to encounter, on account of the medical works of the Greeks not having yet been rendered generally accessible, by good translations [2]; so that, in fact, but

[1] In 1666, a writer speaks of the employment of amulets, as "the cheating *Galenical* doctrine, fetched from *Padua*." *Loimotomia, or the Pest anatomized, by George Thomson, M.D. London*, 1666, p. 161.

[2] It was not till the 17th century that good translations of the Greek and Roman classics began to prevail. The Geneva edition of the works of Hippocrates, by Foesius, was printed in 1657.

few persons were aware that it was a new opinion: and those who were, found it expedient to suppress their knowledge.

When we reflect, that, about the middle of the 16th century, the spiritual authority of the apostolic see was so boundless, that the Duke of Alva, when approaching the gates of Rome, at the head of an imperial army, as a conqueror, was content, instead of pursuing his triumph, to make the most humiliating concessions, in order to be restored to the Pope's favour, and to go personally to Rome, to ask pardon of his holiness, and to receive absolution, in his master's name, and his own [1]; when, I say, we reflect upon these things, we shall not be surprised that even a medical opinion, however palpably absurd, as well as contrary to all preceding doctrines, should, upon such authority, have been instantly received, and rapidly propagated; that it should, with time, have been strengthened, extended, and confirmed; and that, it should even have survived the influence, which gave it birth. By the Roman Catholics, this belief appears to have been universally adopted, as an article of faith; to the Protestants, it has been communicated by

[1] It was upon this occasion that the Pope (Paul IV.) exclaimed, that "before he would lose one *iota* of his due, he would see *the whole world in ruins.*" *History of the Council of Trent, &c. p.* 406.

the sympathy of panic terror; and, if the Turks have escaped the *infection*, it is perhaps partly because, with them, this new opinion has been repelled, by a species of faith, operating in a contrary direction.

In common charity, we are bound to presume, that neither the successor of St. Peter, nor his physician, could have been fully aware of the consequences, which were to result, from the general reception, and establishment, of the medical doctrines, which, for a political purpose, they had caused to be promulgated, under the authority of the apostolic chair. They could not, however, but have foreseen, that, right or wrong, useful or pernicious, so sanctioned, it would become heresy to doubt, and still more to controvert them; that the belief in contagion, in epidemic diseases, would thenceforth become an article of almost universal faith amongst Christians; that it would be deemed an impiety not to be converted; and that physicians, however strong their scientific doubts, would hardly dare to express, even in the most measured language, their incredulity.

In effect, it became extremely difficult for physicians, and medical professors, in the dominions of those states, that had either espoused the new doctrine of the plague being a contagious disease, or were, in any manner, under the

power of the ecclesiastical state, if they regarded their proper dignity, or were not indifferent to the interests of science, to know how to act. Those, who were determined, in contempt of all principle, to be orthodox, as Fracastorius, Forestus, Alexander Benedictus, Sennertus, &c. found, no doubt, profitable employment, in inventing fresh facts, or accommodating facts already known, to the new papal doctrines: whilst those, who could not suffer their complaisance to violate truth, reason, and common sense, as Mercurialis, Rudius, Sanctorius, Septalius, &c. although the field of science lay before them, durst not reap. Who, in the 16th century, would dare, in Italy, then the principal seat of medical, as well as other learning, formally to controvert an opinion sanctioned by a decree of the Council of Trent, and ratified by the Pope? Those, who were of an opposite opinion, were constrained to silence, or merely to hint the novelty of the doctrine now advanced, and their surprise that so obvious a thing, as contagion must be, had not been observed by any of the ancient physicians.

Although it be a matter of very little consequence in itself, to determine what may have been the opinion of the ancients, or what is the opinion of the moderns, upon any given subject, provided we can ascertain and establish the true

one; yet I have thought fit to enter at considerable length into this detail, in order, by divesting the question of that prepossession, which arises from the credit of presumed antiquity, to be enabled to conduct the reader the more readily to just conclusions.

From this examination, it results, as we have just seen, that the doctrine of contagion was utterly unknown to the ancient physicians, which, in a matter so obvious, and, in respect to diseases, of such frequent occurrence, could not have been the case, if it had any foundation in truth; that it has been uniformly taken for granted by the the moderns without examination; that it is, in fact, an innovation, of recent date, and of fraudulent purpose; and consequently possessing no claims to that consideration, which antiquity confers, even upon error.

I have now, I trust, fully succeeded in illustrating the origin of the doctrine of contagion. The question next occurs, to what extraordinary influence are we to attribute the rapid progress, the almost universal adoption, and the permanent establishment, of a doctrine, which is found to be not only destitute of the attractions of truth, but of every vestige of probability, and entitled to no respect from its antiquity, as well as fraught with the most destructive consequences to mankind? In few words, to the influence of

the court of Rome:—to the decree of the Council of Trent, in 1547, first prompted, and afterwards ratified, by his holiness Pope Paul III. removing that council under the pretence of an infectious sickness, to Bologna. This decree, notwithstanding the doubts occasionally hinted by scientific physicians, was quite sufficient to decide the opinions of Italy, in respect to the cause of epidemic diseases; and the opinions of Italy, at that period, in matters of learning, to direct those of the rest of Europe.

The dread of infection being announced, under the sanction of the head of the Christian church, as the ostensible motive of so important a measure as the translation of a general council, contagion, in epidemic diseases, could not fail instantaneously to obtain the implicit assent of at least all the Catholic world; and, almost from that moment, the opinion, excepting with a few enlightened physicians, who however, durst only hint their doubts, may be regarded as having been permanently established. To advocate contagion, was to be well with the Pope, the Cardinals, and the Clergy: and it cannot be doubted, that zealous propagators of a doctrine, which it would be heresy to controvert, would be found to abound, amongst the clerico-medical functionaries, who were looking toward Rome for preferment.

Even in Protestant countries, where greater freedom of discussion prevailed, the influence, in consequence of the state of education, still exercised, over medical affairs, by the court of Rome; a fashion in modes of faith; and the terrors inspired by the actual ravages of epidemic diseases, together with certain effects of self-love, to which I shall elsewhere have occasion to advert, have been more than sufficient to restrain the mere propensity to investigation, and to prolong to our days the duration of ignorance, or rather the pernicious consequences of false knowledge, upon this subject.

Thus has a fraudulent and fatal decree, emanating from the ambitious projects of a Churchman, been productive of greater havoc, amongst mankind, than all the acts of all the conquerors, who have deluged the earth with blood, from Julius Cæsar, to Napoleon Bonaparte [1]!

Had the doctrine of contagion, in epidemic diseases, previously existed, it would have been of little consequence, in respect to our subject, that the Pope should have employed that generally re-

[1] Napoleon, in the plenitude of power, at St. Cloud; trampling upon the liberties of the world, I abhorred, and would have aided the meanest slave in dethroning him. To Napoleon, an exile on the rock of St. Helena, I can render justice, and do not scruple to rank him with the first of the Cæsars.

ceived belief, as a stratagem, to frighten the fathers of the Council of Trent; and to serve as a pretext for translating the Council to Bologna. But it is of great consequence, to the question, to shew, that contagion was, now, for the first time, alledged to exist, in epidemic diseases; that the doctrine was invented, without any foundation in truth, or probability, and without even the pretext of any actual sickness beyond the ordinary diseases of the season, merely for the purpose of this removal; and that the authority, which introduced, was sufficient to establish, to spread, and to perpetuate the belief.

*Illæ sunt tragediæ medicæ, Italiæ imposturæ*, applied by Van Helmont to Amulets, may be much more fitly applied to contagion, and its baleful consequences. These consequences were excellently calculated to render the doctrine permanent. Being, in Christian communities, every where established, by the end of the sixteenth, it continued undisturbed throughout the seventeenth century. Its fate, throughout the eighteenth, will afterwards be the subject of particular consideration.

## CHAP. VI.

*General diseases, which are capable of affecting the same person repeatedly, do not depend upon contagion. Were it otherwise, where no precautions are taken, epidemic diseases would never cease, until communities were extinguished.*

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If general diseases, which are capable of affecting the same person repeatedly, depended upon contagion, consequences would take place, which do not happen.

There would be no possibility, even under the most constant and harassing precautions, of restraining the devastations of those maladies. Every person in health would be attacked, as often as he came within the infectious distance of any one labouring under disease. Those only would escape, who could preserve themselves constantly beyond that distance; or were labouring under some malady higher, in degree, than the prevailing epidemic. But, as disease spread, no person in health could continue long beyond the sphere of infection: and those, who had been protected by diseases higher in degree, would,

upon recovering, become liable to be affected by the contagion. It would observe a geometrical progression, diverging, as it were, from the centre, to every point of the circumference, of a city, a camp, an hospital, or a ship. Whilst it had subjects to operate upon ; i. e. whilst a single individual of the community remained alive, it would never cease. Those, who recovered, would, again, and again, be seized. No person, who remained within the pale of society could escape. The malady would be communicated to the most distant nations. Infection would proceed in a continued circle, until the whole human race was extinguished.

Such would have been the consequences of the contagion of small-pox, had that disease been capable of affecting the same person repeatedly. No precautions could, in that case, prevent its being in a state of constant circulation, whilst it had subjects to operate upon: without precautions, its progress would be still more rapid and destructive. Must we not, then, conclude, that, as the plague is shewn to be capable of affecting the same person repeatedly, if it were also contagious, it would, especially where no precautions are taken, as in Turkey, never cease, until the whole population was annihilated ?

Fortunately for mankind, none of these consequences are found to take place. Epidemic dis-

eases, which are known to be capable of affecting the same person repeatedly, and have, since the middle of the sixteenth century, usually been reputed contagious, for the most part cease, after having affected but a small portion of a community.

These observations apply equally to all general diseases, which are capable of affecting the same person repeatedly, and which have been at any time supposed to depend upon contagion; as dysentery, diarrhœa, and scurvy, when these diseases have prevailed in camps, hospitals, or ships. The last of these, a disease unknown to the Greeks and Arabians, although its cause is distinctly indicated by the time and manner of its first appearance, viz. during the early voyages of the Portuguese to the East Indies, has been attributed by some writers to contagion [1].

Warner, Sydenham, Hoffman, Boerhaave, and Van Swieten, I have somewhere read, have considered even Gout as *sometimes* contagious. The last mentioned Physician cites from German authors two cases of persons, whose attack of that malady were imputed to their having worn shoes, that had been used by a gouty person. It has been even pretended that a dog may catch the disease,

[1] G. Fabricius, in his Antiquities of Misnia, and Sir Gilbert Blane, in his Diseases of Seamen.

from laying at the feet of a person, who has got the gout. Van Helmont says that a woman was affected with this malady, from having sat in a chair, which had been used by her gouty brother [1].

These tales are scarcely fit to rank with the leather cap of Fracastorius, or the feather bed of Benedictus; and as they cannot be accounted for by terror from the maladies, as in plague, and scarcely even by credulity, we must suppose them to be derived from some motive of hope or fear, emanating from the great original source of contagion,— the Court of Rome.

In several countries this idea has been entertained with respect to Consumption: and that disease is almost as much dreaded in the Levant as the Plague itself.

The qualifying "*sometimes*," if even it were not the declared doctrines of the schools, would be a virtual admission, that, *at other times*, these diseases depend upon other causes than contagion.

But it has been shewn that diseases which depend upon other causes, and diseases which are capable of affecting the same person repeatedly,

[1] Bosquillon's French Translation of Cullen's Practice of Physic, Vol. I. P. 330, Note.

are never produced by contagion : and all these diseases are known to be in this predicament.

I have shewn that no such doctrines were held by any Physician, or entered into the superstitions of any people, previous to the 16th century. Were there, then, no fever, no dysentery, no diarrhœa, no gout, no consumption, amongst the Greeks and Romans? Or is contagion so very obscure in its operation, that it could not be discovered by the celebrated physicians of those enlightened nations, and could only be traced by the more exquisite discernment of those of the 16th century?

# BOOK III.

## LAWS OF EPIDEMIC DISEASES.

## CHAP. VII.

*Epidemic diseases are capable of affecting the same person repeatedly—Olivier—D'Ohsson—Russel—Curry, of America—Samoilowitz—Cullen—Sydenham—Burnet—Mac Gregor—Mourad Bey—Wittman—Fra. Louigi di Pavia—Rush—Glasse—Callaghan—Nooth—Burd—Donnet—Muir—Amiel—errors of Mr. Pym and Sir James Fellowes.*

It is one of the principal distinctions between contagious general diseases, and those general maladies, which do not depend upon contagion, that the latter are capable of affecting the same person repeatedly. Plague, and other fevers, which have for so many ages usually been reputed contagious, are subject to this law.

"In the Levant, no one doubts," says a modern traveller, "that a person may have the plague more than once; and the opinion of the physicians, in this respect, coincides perfectly with that of the public.·····Hence, the proposal made by some physicians, to inoculate for this disease, as for the small-pox, is at least ridiculous [1]."

[1] Olivier's Travels in the Othoman Empire, Egypt, and Persia, Vol. I. chap. x.

" Le bonheur d'avoir echappé à la mort," says Mr. D'Ohsson, speaking of persons, who have had the plague, " ne les guarantit pas des nouvelles atteintes de cette épidémie: il en est qui ont la peste plusieurs fois, et qui finissent par y succomber: c'est meme le sort ordinaire de ces empiriques, Mahométans ou Juifs, qui se dévouent à la cure de pestiférés [1]."

Dr. Alexander Russel, in his account of the plague, goes even farther: " The having had this distemper once does not prevent the contracting it again. I have seen instances of the same person being *infected, three* several times, in the same season."

The Mameluke Chief, Mourad Bey, is stated to have fallen a sacrifice to a second attack of the plague, the same season [2].

Fra. Louigi di Pavia, Prior of the Hospital of San Antonia, at Smyrna, mentioned by Howard [3], lately died at an advanced age, after having had the plague, according to general report, at least four times, and, according to other accounts, much oftener.

Dr. Rush, in his account of the yellow fever of Philadelphia, in 1793, confirms the fact in question, as it regards that epidemic: " Cases of *re-*

[1] Tableau General de l'empire Othoman, Tom. II. § vi.

[2] Wittman's Travels, p. 518.

[3] Lazarettos, p. 33.

*infection,*" says he, " were very common, during the prevalence of this fever." He gives the names of six persons, whom he attended in 1797, who had previously undergone an attack of the epidemic [1].

Mr. Glasse mentions the case of Mr. Callaghan, wine merchant, in Irish Town, Gibraltar, attended in 1804 by Dr. Nooth and Mr. Burd, and in 1813 by himself; having, in both instances, nearly lost his life [2].

A man under the care of Mr. Donnet, who had recovered and was discharged, had an attack a fortnight afterwards similar to the first [3].

Mr. Muir, in 1812, had a violent attack of yellow fever, in Surinam. In 1813, he was one of the first attacked at Gibraltar, and died on the fourth day with yellow suffusion, and black vomit. His clerks, who attended him in his illness, were made to perform quarantine, but remained in perfect health [4].

Mr. Amiel had two cases of second attacks under his own care, and he states his being informed, on good authority, of several others [5].

Fifty-six instances of second attack are enumerated as having been observed, by different medical officers of the navy and army, at Gibral-

[1] Med. Enq. and Obs. Vol. V. p. 36. [2] Burnet p. 333.
[3] Burnet, p. 333. [4] Id. p. 334.
[5] Ibid. p. 334.

tar, in twenty-two of which it occurred in the same, and in thirty-four in a different season [1].

Sir James Macgregor states, that "on the 1st of August, 1796, the first case of yellow fever made its appearance on board the Betsey transport, from which period to the 12th of September, almost every person on board was attacked once, and a great many *twice* with this fever [2]."

Dr. Curry (of America) says: "It is asserted by Dr. Lining, that those who have once had the disease (yellow fever) cannot take it a second time; but we have seen several instances of its occurring here a second time, where the circumstances were so unequivocal, that it could not fairly be ascribed to a relapse [3]."

Mr. Samoilowitz, in his account of the plague, which carried off sixty thousand of the inhabitants of Moscow, in 1771, states that he was himself attacked *three* times by the disease [4]. Yet this writer has filled nearly one hundred pages of his work with what he calls *proofs* of its contagious influence. As, in reports of trials at law, swearing; so, in reports of medical proceedings, asserting, is often mistaken for proving.

As this fact forms one of the principal grounds of my conclusions, respecting the cause of epi-

[1] Burnet, pp. 468—470. [2] Medical Sketches.
[3] Treatise on the Synochus Icterodes, p. 15.
[4] P. 35, 39, 40.

demic diseases, it was necessary that I should establish it beyond dispute. But it would be superfluous farther to multiply evidence, in order to prove what is not denied, even by the advocates for contagion, who are themselves indeed my principal authorities, on the occasion.

But attempts to prolong the existing delusion, respecting the cause of epidemic diseases, chiefly upon fallacious reasoning, connected with this fact, having recently been made, by writers, to whom some authority has been attributed, upon the subject, I am induced to add a few observations, which I should not otherwise have considered necessary, in illustration of the futility of such reasoning, and the fallacy of such conclusions.

Mr. Pym, who has had the charge of quarantine establishments in the Mediterranean, has endeavoured to shew, by a species of induction, which may be regarded as a curious specimen of medical logic, that the fevers, which have of late so frequently afflicted Gibraltar, and the towns of Spain, have depended upon a specific contagion. This he has attempted to effect, by confounding the *fact* of the disease *not affecting*, with the *principle* of its *incapability to affect*, the same person, more than once[1]. This capability is

[1] Pym's Observations, &c. pp. 28, 56, and 69. Fellowes, p. 67. Arejula on the yellow fever of Andalusia, p. 191.

various, in various epidemics: and in some, the repetition occurs so rarely, that, were we to judge from this circumstance alone, we might often be led erroneously to conclude that they belong to the class of contagious diseases. But other criteria are seldom wanting to direct our judgment with certainty: as the absence of some phænomena peculiar to diseases, which depend upon contagion, and the presence of others, which are proper to epidemics.

In scarlet fever, for instance, we seldom meet with a person, who has had the disease more than once. But it would be rash to conclude, from this circumstance alone, which however has been done, that it is incapable of occurring repeatedly in the same person, or that it depends upon contagion; especially, when we find that it is undoubtedly subject to laws, which are proper to epidemics. Sydenham observed, that, it appears, in England, after the winter solstice, and disappears after the summer solstice. Cullen says, that, in Scotland, it appears generally about the commencement of winter, and continues to reign, during that season. He has known it to be epidemic, forty five times, in Scotland.

The same observations will apply to that species of epidemic, which affects particularly persons newly arrived, in hot climates, and has

therefore not unaptly been denominated the "seasoning fever." It is occasioned principally, by exposure to the sun, and other powers of intense action, in persons unaccustomed to the operation of these agents in so high a degree. The habit however being established, or the disease having once occurred, it will afterwards require a higher sum of power, to produce in the same person similar effects: and hence, the same species of fever rarely occurs again in the same person. But, it does not, by any means, follow, that, the liability to it does not exist; or, that, by increasing the force of the cause, in the ratio of the habit established, or of the susceptibility lost, in consequence of the first attack, it would not be produced, as before. In this case, however, it would no longer retain its name of "seasoning fever." *As such*, it is certainly incapable of occurring, more than once, in the same person. But no one dreams of considering it, on this account, as contagious.

Not only does the liability to the same species of fever diminish, in proportion as persons become seasoned to the climate, in which it occurs, or after having the disease once; but, considering fevers as of distinct species, which however, as I shall elsewhere shew, is not correct, the having had one kind of fever, diminishes the li-

bility to another. The very circumstance of their having been seasoned, by a residence in the West Indies, would have tended to secure the soldiers against the fever of Gibraltar; and their having had any species of fever, in that, or any other country, would tend still farther to increase that security. But, it was too much to conclude, because seasoned soldiers, and soldiers, who had had fevers, in other countries, were not so liable to be seized with the epidemic of Gibraltar, as the more raw hands, or because they might have totally escaped, whilst those raw hands were suffering, that, the fever was therefore one, which occurs only once, in the same person; that it was, consequently, contagious; and, that the contagion was of Bulam origin, circuitously imported from the West Indies. These suppositions would have been far too violent, if it were even admitted that contagion could exist, as a cause of epidemic diseases. And they are also unnecessary toward accounting for the rarity of recurrence of any disease in the same person, since that is equally explicable, by the laws of habit, and indeed more so, if it be supposed to depend upon the influence of the atmosphere.

Sir James Fellowes, upon this subject, falls into a similar error[1]. That the fevers of Spain

[1] Reports, Introd. p. xxiii.

have not depended upon contagion, inevitably results from the demonstrated fact, that they have all commenced and ceased at certain determinate periods. And, upon this ground, if there were no other, they must be presumed to have been capable of affecting the same person repeatedly, however rarely that occurrence might have happened.

## CHAP. VIII.

*Epidemic diseases commence and cease at periods corresponding with certain changes of the seasons, and differing in different countries, according to the periods of these changes respectively. Ordinary periods in Asia Minor, Egypt, and Syria—in parts of Europe, and of North America—of the East and West Indies—their commencement and termination, in different years, in Constantinople, Aleppo, Smyrna, Paris, Marseilles, Cadiz, Carthagena, Medina Sidonia, Leghorn, Gibraltar, London, Moscow, the cities of America, and the islands of the West Indies.*

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Although, in countries liable to frequent returns of epidemic diseases, straggling cases are occasionally to be met with, at all times of the year, it is only at particular seasons that they occur with the severity which constitutes a pestilence.

In some countries, as Asia Minor, Egypt, and Syria, the pestilential period is from April to July; and, in others, as most parts of Europe, and North America, from July to November. This period varies according to geographical position, and is anticipated or postponed by circumstances.

Of the Plague of the Levant Dr. Russel has observed: "The winter puts an end to it at Constantinople: the summer destroys it in Egypt."

At Aleppo, according to the same writer, the European inhabitants used regularly to shut themselves up in their houses, from April to July.

At Smyrna, according to Fra. Louigi, "the plague is most fatal from April to July [1]." A Jew physician of that place states, that, commonly, "the disease appears at the end of spring, and continues to the middle of summer, with this particularity, that, in cloudy weather, and during the Sirocco wind, the attacks are more frequent [2]."

In Egypt, the plague generally ceases about St. John's Day, (the 24th of June) [3].

In Paris, in 1533, the plague prevailed in September, as we learn from the Ordinance of Police, of which the title has been quoted in a preceding chapter.

At Marseilles, in 1720, it also occurred in the autumnal months [4].

At Cadiz, the epidemic of 1764, according to Dr. Lind, prevailed in September and October [5]. That of 1800, commenced in August [6], and ceased toward the end of October. That of 1804 commenced in August, and ceased early in December [7]. That of 1810 commenced in September [8], and ceased in December [9]; that of 1813 commenced

[1] Howard, p. 33. [2] Id. p. 35. [3] Wittman, p. 529.
[4] Journal of the Plague of Marseilles in 1720. [5] Lind. P. 122.
[6] Sir James Fellowes, p. 33, and 46, 7. [7] Pym. p. 32.
[8] Burnet, p. 214, 215. [9] Sir James Fellowes, p. 237.

early in September [1], and began to decline on the 12th of November [2].

At Carthagena, in 1785, "they lost, during the autumnal months, 2500 persons, and the succeeding year 2300 [3]."

The epidemic of Carthagena in 1804, commenced in September, and ended in January, 1805 [4]. Twenty thousand perished out of a population of thirty-four thousand. In 1810, it commenced early in September [5]; and swept off three thousand persons, being a fourth part of the inhabitants, in the course of six or eight weeks [6]. In 1811 and 1812, that place was again visited, at nearly the same periods, by this malady, which made great ravages [7]. In the latter year, two-thirds of those taken ill, died [8].

The seasons in Carthagena are as in the Mediterranean generally; warm weather commencing in May and June, increasing in July, August, and September, continuing during October, and sometimes for a week or two in November. The heavy rains fall commonly in September. The thermometer in August and September stood in the shade at 85 [9].

In Medina Sidonia, in 1801, the disease raged

[1] Sir James Fellowes, Introd. p. xiv. and p. 256. [2] Id. p. 296.
[3] Townsend's Journey through Spain, Vol. III. p. 136.
[4] Burn, p. 219. [5] Burn, 274. [6] Id. 220.
[7] Id. P. 219. [8] Id. 275. [9] Id. 222.

from the middle of August to the commencement of November [1]. During this period all the sea-port towns of Andalusia enjoyed the most perfect health.

In ten, of the twenty-three towns, of Spain, which were ravaged by the epidemic of 1804, the disease began in August; and, in eight, in September. In sixteen, the greatest mortality happened in October. Eight were declared healthy in November, and twelve in December [2].

At Leghorn, in 1804, the yellow fever prevailed in the autumnal months [3].

The epidemics, which afflicted Gibraltar, in 1800, 1804, 1810, 1813, and 1814, prevailed in the autumnal months, and all commenced and ceased, at similar periods, or periods nearly similar, there, and in all the places which they affected, in Spain, and Italy.

"Mr. Gardiner, who has been ten years surgeon of the Naval Hospital at Gibraltar, assures me," says Dr. Burnet, "that fever always shews itself less or more in the garrison during the autumnal months, and particularly so when the rains are slight and succeeded by hot weather [4]."

In September, 1810, a fever amongst deserters

[1] Burn, p. 493.

[2] Sir James Fellowes, Table, p. 478.

[3] Recherches Pathologiques sur la fievre de Livourne de 1804, par Tommasini, &c. p. 14.

[4] Burnet P. 205.

from the French armies, and sailors from England, appeared at Gibraltar [1].

The Leyden arrived with the 67th Regiment at Carthagena in the end of August, at which time what the inhabitants call the yellow fever prevailed there.

In the fleet, as well as on shore, the fever was found to be most severe in autumn [3].

At a station called St. Julian, not a man was attacked. And, on board the Leyden, in the harbour, although forty of the crew and soldiers, who had been ashore, were affected, not one, who had not quitted the ship, was seized. Of 250 of the 67th Regiment, attacked on shore, seventy died, and thirty were invalided [4].

At Malta, and in Wallachia, in 1813, the plague began, increased, declined, and ceased, at similar periods; the former having a police and quarantine establishments of the most perfect kind, and the latter none. The greatest mortality occurred in both places in the months of July, August, and September [5].

The following view of the periods of greatest mortality, from the plague, in London, principally in the seventeenth century, and in other places more recently, may serve to illustrate this position, in as far as these epidemics are concerned.

[1] Burnet p. 214, 15. [2] Id. p. 226. [3] Id. p. 253.
[4] Id. P. 225. [5] Chapter on the epidemic of Malta.

| | Year. | | Week ending | Greatest No. of Deaths. |
|---|---|---|---|---|
| London, | 1592 [1] | ending | August 4 | 983 |
| ——— | 1603 [2] | | Sept. 1 | 3035 |
| ——— | 1625 [3] | | August 18 | 4463 |
| ——— | 1630 [4] | | July 29 | 77 |
| ——— | 1636 [5] | | Sept. 29 | 928 |
| ——— | 1665 [6] | | Sept. 19 | 7165 |
| Marseilles, | 1720 [7] | Months of August and September. | | |
| Gibraltar, | 1804 [8] | on the 9th day of October | | 170 |
| ——— | 1813 [9] | on the 17th day of October | | 37 |
| Malta, | 1813 [10] | Months of July, August, and September. | | |

All the epidemics, in Great Britain, of which we have any certain records, prevailed in autumn, and committed their greatest ravages in August, September, or October.

The fever called Sudor Anglicus, or Febris ephemer. Britan. has appeared at different periods, in that season. In 1713, it commenced in September [11].

In London, in 1625, the greatest mortality took place in August. The last week of that month, there died 4218; the following week 3044; and the third week, only 852 [12].

[1] [2] [3] [4] [5] [6] The Plague of Marseilles considered, by Richard Bradley, F.R.S. London, 1721, p. 14, et seq. and Tables of Mortality in the Appendix.

[7] Journal of the Plague at Marseilles.

[8] Burnet, p. 205. [9] Id. Table at p. 341.

[10] Ch. on the epidemic of Malta. [11] Caius, Mead, Friend.

[12] The shutting up of houses soberly debated, Anno 1665.

In 1665, of 68,590, the whole number that perished of that malady, in the metropolis, according to the bills of mortality, there died 49,705, being considerably more than two thirds, in the two months between the 8th of August, and the 10th of October [1].

The autumn is also the epidemic season in America. According to Dr. Rush, "the yellow fever appeared six different times about the first, or middle of August, and declined or ceased about the middle of October, viz. in 1732, 1739, 1745, and 1748 in Charlestown; in 1791 in New York, and, in 1793, in Philadelphia [2]."

The plague which afflicted Constantinople in 1812, took place as usual in the autumnal months. "During our stay at Larissa," says Dr. Holland, "the archbishop received a letter of some credit from that city, in which it was affirmed, that the deaths there in the preceding three months, amounted to about a hundred and twenty thousand; and that in the month of October not fewer than two thousand on the average died every day [3]." This, like such accounts in gene-

[1] History of the Plague by H. F.

[2] On the Bilious Remittent Fever. &c.

[3] Travels in the Ionian Isles, Albania, Thessaly, Macedonia, &c. during the years 1812 and 1813. By Henry Holland, M.D. p. 265.

ral, is probably beyond the real number: but absolute correctness is not here necessary.

The plague of Moscow in 1771, happened in the autumn. Some straggling cases occurred throughout the winter: but De Mertens says, that, sickness and mortality considerably decreased after October; and Orræus states that the deaths, in September, were 21,404; in October 17,564; in November 5,235; and in January only 330[1].

Samoilowitz acknowledges that, but very few even of the persons who had been newly entered in the hospitals for the service of the sick, had the disease after the month of November; and none but in its mildest forms.

In the West Indies, the epidemic season, in some of the Islands, as Grenada, is nearly the same with that of Egypt, Asia Minor, and Syria, &c.; in others, as Jamaica, it is nearly the same with that of most parts of Europe; and, in others, as Dominica, it observes an intermediate period. The fever in Grenada in 1793, 4, and 5, commenced in February[2]: that of Antigua prevailed in March and April[3]; that of Guadaloupe in May[3]; and that of Dominica, in the same years, in June[4]: The difference of periods, other things

[1] P. 48. [2] Chisholm.
[3] Pym's Observ. p. 186, 7.
[4] Treatise of the Yellow Fever, by James Clarke, M.D. p. 49.

being equal, will perhaps be found to be in proportion to the degree of latitude. But there, of course, as happens every where else, the ordinary periods are occasionally anticipated, or postponed.

At Demerary, in 1800, a destructive fever prevailed in the month of *August*, which Dr. Chisholm affirms to have been produced by contagion *imported on a ship from Liverpool*[1]. The other West Indian Islands have often been afflicted with epidemic diseases, and on many occasions, it may be presumed, as severely as Grenada, although they have not had such historians as Dr. Chisholm. In 1793, indeed, the sea port towns were generally visited with the epidemic of the season in various degrees; seldom affecting the interior.

During eight years residence in Jamaica, Mr. Doughty had observed, that, "although fever is occasionally produced in every month of the year, yet that particular order of it, denominated endemic or yellow fever, is rarely seen till the autumnal season, viz. from the beginning of August to the latter end of December[2]."

[1] *New York Med. Rep. Vol. V. P.* 229.

[2] Observations and Enquiries into the nature and treatment of the Yellow, or *Bulam* Fever, *in Jamaica* and *at Cadiz*, &c. By Edward Doughty, Surgeon to the Forces, &c. 1816.

The 6th Battalion of the 60th Regiment, upwards of eight hundred strong, from the beginning of February to the middle of August, 1801, lost but one man. In the three months which followed, they lost nearly two hundred men [1].

In Jamaica, the rains generally set in, in August. With the advance of vegetation, which then commences, diarrhœa, dysentery, and fever arise in a sort of succession. The latter are "first of the mild remitting order, and progressively advancing to the violent, ardent endemic; or, as it is more generally denominated, Yellow Fever [2]."

It is a remarkable circumstance concerning the epidemic of Spain, in 1804, that, in three of the principal towns, Cadiz, Gibraltar, and Alicant, the greatest mortality happened upon the same day; viz. the ninth of October [3].

[1] Doughty's Obs. and Enq. pp. 1, and 5. [2] Id. p. 9.
[3] Sir James Fellowes's Reports, Table, p. 478 and 479.

## CHAP. IX.

*The phænomena of epidemic diseases are various and dissimilar; their symptoms observe no regular concourse or succession: there is generally no discernible connection between the application of their cause, and the appearance of its effects; and their duration is indeterminate.*

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Although a general disease, depending upon contagion, as small pox, may differ almost infinitely in degree, from that which is denoted by the fewest discernible spots, to that which is indicated by the most numerous pustules; yet there is neither variety, nor dissimilarity, in the concourse of symptoms, nor uncertainty in the periods of their appearance, or duration, rendering their nature doubtful, or giving the semblance of diseases of a nature altogether different.

In those respects, epidemic diseases are essentially different. There is, not only in respect to the pestilential maladies, which occur in different countries, but to those which occur in the same country, at different seasons, and even to those which affect different persons, in the same country, and in the same season, such a great diversity of phænomena, depending upon the variety of com-

binations, and degrees, in which the different organs are affected, in different individuals, as to give to the same pestilence the semblance of many maladies of a dissimilar nature.

In the plague of Marseilles, in 1720, for instance, some of the French physicians imagined they could perceive five distinct species of the disease [1]. In Smyrna, they affect popularly to speak of fifteen kinds of pestilence, of which the most aggravated form is there denominated "*the mother plague.*"

This great diversity of phænomena has every where procured for the plague the epithet of a *Proteian* malady : and hence also the misleading appellations given to the different forms of pestilence, derived from some predominant symptom, its most ordinary type, the most usual place of its appearance, or some combination of several, or all of them ; as yellow fever, bilious fever, remitting fever, bilious-remitting fever, bilious-remitting-yellow fever, sweating-sickness, scarlet-fever, Dunkirk fever, Bulam fever, Gibraltar fever, Mediterranean fever, Jungle fever, Hill fever, Fen fever, &c. &c. &c.

Hence, during the epidemic of Philadelphia in 1793, it was thought by some that every disease

[1] Observ. et Reflect. touchant la Peste de Marseille, p. 47, et suiv.

had resolved itself into yellow fever, whilst, by others, it was affirmed that the diseases of the city were various [1]. It must be obvious, that, although pre-existing diseases will, according to the intensity of its cause, be merged in the prevailing epidemic, every usual variety of disease unless that be of extraordinary severity and diffusion, will still continue to prevail in a community.

The difference, in respect to diversity of symptoms, between epidemic and contagious diseases, is explained by the difference in the nature, and manner of application, of the powers, which produce them. The air, constantly and diffusively applied to the living body, and indispensable to its existence, never operates, for any length of time, upon the same, or upon different individuals, with uniform intensity. The results of its operation are different, according to the proportions and combinations of its component parts, and to its impetus. They are different in a calm, and in a storm; in different countries, districts, towns, streets, houses, rooms; and even in different corners of the same room, according to the circumstances, as connected with these situations, which affect the qualities of the atmosphere, particularly motion, temperature, moisture, &c. Whence, we may understand the reason of the

[1] Rush on the Bilious Remitting Fever.

great, but merited importance, which Hippocrates has affixed to air, water, and situation.

The customary degrees of deterioration of the atmosphere, which take place in ordinary seasons, are productive, in a few persons, in a slighter degree, of those diseases, which, when more diffused, constitute an epidemic, and when more mortal, a pestilence. Upon the qualities, and the impetus of this power, varied and modified almost *ad infinitum*, and upon the previous state of the general excitement, or of the excitement of particular organs, respectively, will depend the numerous affections of the different organs, and the different degrees of affection of the same organ, which are indicated by every possible combination, concourse, or succession, of symptoms, that distinguish epidemic maladies: and these phænomena, although the affections, which they indicate, can only differ in extent, situation, and degree, are almost infinitely diversified. From these causes, then, arise not only the affections, which are known by the names of catarrh, rheumatism, quinsy, peripneumony, pleurisy, dysentery, diarrhœa, scurvy, common fevers, spasms, convulsions, paralyses, &c.; but those higher degrees of disease, denominated plague, and pestilence.

Such is the manner of the application of the powers, by the undue action of which epidemic

diseases are produced, that, different from contagious matter, poisons, and remedies, no distinct connection can be traced between the period of their noxious operation, and the commencement of disease. The progress of the diminution of excitement, from the healthy standard, through all the stages of what has been usually called pre-disposition, to the degree which constitutes palpable disease, is so various, according to the different circumstances, in which individuals are placed, and to the different degrees of exciting power of the atmosphere, that a disease of this kind may be forming for weeks or months, or may be formed in a day or an hour. When a person is placed in air, of which the power is diminished below the degree capable of sustaining life, as in air highly deteriorated, death of course instantly follows. But between that extreme degree of noxiousness, and that, which is capable of producing a slight catarrh, not only must the degrees of affection, and the phænomena, which this power is capable of producing, be almost infinitely various; but the periods, during which, in a season of pestilence, the prevailing disease is forming, must, in different individuals, be very different. Hence the contradictions, and perplexities, of those, who have endeavoured, under the notion of contagion, in epidemic maladies, to assign the period be-

tween receiving the *infection*, and the appearance of the disease. Demoullins says, "it is two or three days[1];" Giovanelli, "there is no certainty, as it depends upon the constitution of the patient[2];" They, "the interval from the infection to the seizure is various, sometimes it acts *slowly*, sometimes *like a stroke of lightning*[3];" Verdoni, "generally the disease shews itself *at the instant of the touch*. Sometimes it does not appear *for several days*[4];" the Jew physician, "the contagious miasma may lie dormant in the body for some time, without doing the least harm till set in motion[5];" Fra. Louigi, "the infection shews itself in twenty-four hours, *more or less*, according to the difference of temperament[6];" Julius Cæsar Kelli, "it does not appear till after the second or third day[7]."

Samoilowitz states the interval between the infection and the appearance of disease, as extending from two to fifteen days inclusively. He might with equal reason have said from twenty minutes to twenty years.

Dr. Bancroft, in one or two instances which occurred at Aboukir, "was *inclined to believe* that the disease had been produced within twenty-four, or at most, thirty-six hours, after the con-

[1], [2], [3], [4], [6] Howard, p, 35. [5] Ibid. p. 33.
[7] Manuscript observations on the plague at Brusa, &c.

tagion had been applied[1]." INCLINED TO BELIEVE! such is the kind of evidence upon which doctrines of extraordinary influence upon the best interests of nations, have been received.

Franciscus Vallolaeri, says he, has seen persons falling down with the plague, *a few hours, paucis post horis*, after having been exposed to its contagion!

But the most extraordinary latitude, which has ever been given, by any person, to any supposed contagion, is perhaps that which has been assumed by Dr. Bancroft for the supposed contagion of typhus. He states, that, upon the return of the British troops from Spain, in 1809, of the orderlies and nurses, who attended the sick, thirty-five, who had been on the expedition, were seized almost all on different days, from the first to the forty-fourth day; it being rarely that more than one was seized upon the same day; and ninety-nine, who had not been out of the kingdom, were seized at various periods, with a considerable degree of equality, from the thirteenth to the sixty-eighth day, and some even later. But, as if this were not enough, he adds, "though *there may be reason to conclude* that febrile contagion does not remain *inactive* so long after being received into the body, as marsh

[1] Essay, p. 594.

miasma, I *see none for believing* that an interval of five or six months, may not *sometimes* elapse before the actual production of fever by it; and I cannot help strongly *suspecting* that such a postponing of the disease happened to some of the troops from Corunna, in 1809[1]." Is this a language fit to be employed in disquisitions of science? Into the question of marsh miasma I shall not here enter. Would it not have occurred to any person, excepting such as had predetermined that contagion *only should* be the cause of this disease, that no contagion could possibly be so capricious in its periods of producing its effects; but that the difference of the periods at which the disease occurred, is easily to be explained by the different degree of excitement, which the individuals affected might have respectively enjoyed, and the different degrees of intensity, with which the causes of the disease might have been applied to them. The debilitated frames of those, who had returned from Spain, were unable to withstand the cause so long, as the more vigorous constitutions of those who had not been out of the country: a difference, which I presume no man will contend would have happened, had the cause, as in small-pox, been contagion. Indeed, it would,

[1] Essay, p. 515—517.

in my opinion, have been extremely difficult for Dr. Bancroft, whom I willingly admit to be an ingenious man, to have discovered a more apt illustration, if his intention had been to disprove the doctrines, respecting plague and typhus, which he was advocating.

If the disease were really contagious, no such perplexity could exist. The period between the application of the virus of small-pox, and the appearance of that disease, is determinate; and their connection obvious. Poisons, spirituous liquors, mercury, cathartics, &c. will commonly produce their appropriate effects within a certain given period from their application: and, when they do not, it is because the organs are not in their ordinary state. Thus wine will not ebriate persons labouring under typhus, because typhus is a disease of higher degree, than that which is denoted by ebriation: mercury will not salivate a person, whose liver is in a state of ulceration, or disorganised, because ulceration, or disorganisation of the liver is a disease of higher degree, than that which is denoted by salivation.

## CHAP. X.

*Epidemic diseases prevail most,* cæteris paribus, *in countries the least cultivated—they affect some classes of the community more than others, according to difference of residence, affluence, modes of living, prejudices, or speculative opinions, occupations, habits, sexes, ages, &c., in proportion as these circumstances contribute to expose, or to render them more susceptible to the operation of their causes.*

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It is one of the most surprising inconsistencies, connected with this subject, to suppose that a disease can be at once contagious, and capable of entertaining a preference for a particular country. Plague being in modern days a frequent visitor of the Levant, and contagion in epidemic diseases being also a doctrine of modern invention, it has of course been assumed by the believers in it, *i. e.* by all Christian communities, that this disease is endemic of, or peculiar to, that part of the world. In order to account for this preference, as one absurdity generally requires the support of several, it was assumed that the Turks, who are without exception one of the most cleanly people upon earth, are exceedingly dirty; and, that, to this dirtiness is owing

the frequent recurrence of the plague among them [1].

But, it ought to be recollected, that this preference has not always been enjoyed by Turkey; but that countries, in this respect, have changed characters, even since the date of the doctrine of contagion. England, France, and all the northern countries of Europe, were, at former periods, probably as much more liable to pestilence than Turkey, as Turkey is at present more liable to pestilence than them.

The reason of this change is obviously to be found in the comparative state of those countries, at former periods, and at present, with respect to cultivation. It is unquestionable, that, for the last three centuries, the nations of the north of Europe, generally have been advancing, whilst those of the Levant have been retrograding. Some of them, however, have either been stationary, or made less progress than others: and, accordingly, we find the provinces of Spain, some parts of Italy, the old Venetian provinces of Dalmatia, Istria, &c., many parts of Poland, and the eastern frontiers of the Austrian dominions, as Hungary and Transylvania, little less liable to epidemic diseases than formerly, not because they are adjacent to Turkey, as has been

[1] D'Ohsson. tom. II. p. 220.

inferred in conformity with the belief in contagion, but because they are in so backward a state of cultivation.

"At Diarbeck and Mossul," says Olivier, "this distemper is not known above once in fifteen, eighteen, or twenty years. It is still less frequent in Bagdad and Bassora; and the Persians are scarcely ever afflicted with it[1]." The French traveller attempts to account for this in the true spirit of a believer in contagion. "What occasions this exemption," says he, "is that those cities hardly receive any goods indigenous to Smyrna and Constantinople: and the pestilential venom has time to evaporate on its long passage through Asia Minor, Mesopotamia, or the deserts of Arabia. Besides it appears that a somewhat severe frost, such as is apt to prevail in Asia Minor, or a great heat, like that experienced in Egypt, Syria, or the desert of Arabia, are sufficient entirely to stifle the germ of this scourge[1]."

In the same manner, he attributes the exemption of Persia from the plague to its little intercourse with Turkey. Even if the premises were here correct, which is not the case, the conclusions would by no means follow. There is quite sufficient commerce between these coun-

[1] Travels in Egypt, &c. chap. xx.

tries to communicate any disease that was really contagious. And if the plague could be so communicated, how can it be explained, that Malta, which has been in such frequent intercourse with Turkey, and so adjacent, should only have had that malady three times in the course of three centuries, viz. in 1592, 1676[1], and 1813; whilst Gibraltar, which is at a greater distance from, and has fewer commercial relations with Turkey, has been four times afflicted with the disease, from 1800 to 1813, or in the space of thirteen years? With respect to the protection derived from plague police regulations, the two places are precisely in the same condition.

In endeavouring to shew that the plague, in Egypt, does not depend upon the inundations of the Nile, Olivier admits that it recurs at particular periods. "The Nile," says he, "begins to overflow toward the middle of Messidor (the beginning of July), and the inundation is complete in the beginning of Fructidor (19th of August). In Vendemiaire (from the 23d of September to the 22d of October), the land is sown which this river has inundated. This of course ought to be the epoch, at which the plague should appear, were it really occasioned by any putrid exhalations, produced by the stag-

[1] Vide Boisgelin's History of the Knights of Malta.

nation of water on the soil; and yet we observe that this malady always ceases in Egypt, during the hottest season of the year, and, appears very rarely in autumn, but most frequently in winter and spring, that is to say, when the waters spread over the soil have entirely disappeared, and when there cannot be any dangerous exhalations. A farther proof that the periodical inundations of the Nile have no influence on the plague, is the total exemption of Egypt from the ravages of this scourge, for several years consecutively [1]."

Upon this passage, I may observe, that it is precisely at the period, at which the waters disappear from the face of the soil, that the exhalations, in as far as they may be a cause of disease, may be expected to become the most noxious. It is, accordingly, at this period that the plague generally commences in Egypt; and it is when the waters again are beginning to overflow the land that it generally ceases. But whilst it is highly probable that these circumstances are, to a certain degree, in the relation of cause and effect, it is also obvious, from the non-appearance of the disease sometimes for several years consecutively, that the changes usually connected with the rising and falling of the Nile, are not

[1] Chap. xv.

alone sufficient to produce the malady. But, on the other hand, this occasional exemption, as well as the recurrence of the disease at particular seasons only, whilst the commercial intercourse with Turkey remains always the same, afford the most conclusive evidence, that it is not propagated from the one to the other, by means of contagion.

The same traveller gives us the following fanciful description of what he conceives to be the progress of this virus: "The plague overruns Turkey, and appears more or less frequently in a town, in proportion to its commercial communications. Thus, for instance, it prevails almost constantly at Constantinople, *because* that city has the most frequent intercourse with all points of the empire." These statements are by no means correct. The disease, although very frequently at Constantinople, does not prevail "almost constantly." Neither has that city the most frequent, if he mean commercial, intercourse with all points of the empire: nor are places of the least commercial intercourse the most exempt from plague.

Again: "It does not break out in any provincial town, without being transmitted to the metropolis." This is quite erroneous. It frequently occurs, in many provincial towns, having direct intercourse with the metropolis, without

the latter being affected: and, indeed, it never occurs there, or any where else, with great severity, but at its usual seasons.

"Smyrna," says he, "is the next place, where this scourge often commits the greatest havoc, *because* it carries on a very brisk commerce, and has frequent communication with almost all the towns in Turkey." The periods of its recurrence at Smyrna, are, however, still more distinct: and, as the commerce and intercourse of that with other towns are not less, during that part of the year, at which the disease never occurs with severity, than during the part, at which it does occur, it cannot possibly depend upon such intercourse.

"The trade of Egypt with Constantinople is considerable. Hence it commonly happens that the Turkish ships, or the caravellas of the Grand Seignor, carry the plague to Alexandria, from whence it spreads to Rosetta, Damietta, and Cairo; and, from thence to all the villages, even into the very dwelling of the husbandman."

If this were the mode of its conveyance, as Turkish ships, and the caravellas of the Grand Seignor arrive in Egypt at all times of the year, the plague would never cease.

Were its introduction at particular periods, capable of being satisfactorily accounted for in this manner, we should still find it impossible

upon the same principles, to explain its periodical cessation. We should in that case be obliged to suppose that it is regularly re-exported, in June or July, from Egypt to other countries? This is too palpable an absurdity to be maintained; but it is alledged that its activity ceases! This is certainly a very easy mode of accounting for any thing. But it is saying no more than that the disease ceases, because it ceases. By what means is its activity diminished? We find that it begins and ceases at periods surprisingly regular. It ceases too, for the most part suddenly, when its extent and mortality have arrived at their highest point. Thus, in the plague of London, in 1665, it was whilst from 30 to 40,000 persons were affected, and when between seven and eight thousand were dying weekly, there being still between two and three hundred thousand persons who had not yet had the disease, that, in the month of October, the malady suddenly declined, and almost wholly ceased. If it had been contagious, there was here more than enough of surface for contagion to infect the universe. By what wonderful influence then, can we suppose the infectious matter with which so many bodies are presumed to be impregnated, and having attained its highest degree of virulence, as well as its greatest extent, to be all at once rendered incapable of propagating infec-

tion? It is wholly inconceivable. If this almost instantaneous, and universal effect, be not the result of an agency universally operating, as the qualities of the atmosphere, then must we abandon natural causes, and return to the exploded doctrines of magic and miracles!

"This cruel disorder," he continues, "extends to Syria, by means of the merchandise, with which that country is furnished from Egypt. It gets likewise thither through Smyrna and Constantinople. From that province it will often diffuse itself over Damascus, Aleppo, and Mesopotamia. The caravans from Smyrna to Constantinople convey it to the interior parts of Asia Minor. Turkey in Europe is more exposed to its ravages, than the distant provinces of Asia, *owing to its vicinity to, and relations with the capital.*"

The fact here assumed, for which, although it be not wholly correct, there is however some foundation, will admit of a very different, and much more satisfactory explanation. The European provinces of Turkey are by far the most backward in cultivation, and otherwise, the least healthy portions of the Turkish dominions.

In the preceding description, Olivier appears to have adopted, without any very scrupulous examination, such of the antiquated traditions of the

Levant upon this subject, as he was enabled to collect, in the course of his travels. It may serve as a specimen of all the narratives of this kind; in which the premises have been regularly taken for granted, and the conclusions as regularly erroneous. 1. It is supposed that contagion is the cause of plague: 2. That that disease is capable of being communicated by contact from one person to another: 3. That it is capable of being communicated from persons to goods: 4. And from goods again to persons: 5. That the frequency of the plague, in any country, is in proportion to its intercourse with Turkey: 6. That the pestilential venom evaporates on its passage through the deserts of Arabia: 7. That a severe frost, as in Asia Minor, a great heat, as in Egypt, and a copious dew as in Smyrna, stifle the germ of the malady.

These speculations, however amusing in the mouth of a traveller, will, in a view of science, be found entitled to no more credit than the Arabian Nights Entertainments. The matter of plague, as represented in these narratives, is a most handy and pliable agent. It is capable of being dissipated by wind, evaporated by heat, congealed by cold, and diluted by moisture. But, like Antæus, it is always found to rise with renovated vigor, from the earth; and notwith-

standing its frequent overthrow, by the hostility of the elements, as often to recommence its ravaging career.

Under a full acquaintance with the whole of the subject, all these suppositions and conjectures will appear palpable absurdities.

Thus we see that none of the leading phænomena, respecting epidemic diseases, are rationally to be accounted for, by the doctrine of contagion; whilst, by that which presumes them to depend upon the state of the atmosphere, and other concurrent causes, they are all easily explicable. There can, according to this doctrine, be no difficulty in accounting for the commencement of epidemic diseases, in some places, in autumn, in other places in spring; nor for their cessation, in some places, in winter, in other places in summer. Vicissitudes of weather being most remarkable in spring and autumn, these are accordingly the seasons most fertile of diseases, in almost all parts of the world. Summer and winter, on the contrary, being more uniform in temperature, are in general, the seasons most favourable to health.

But this cannot depend simply upon degrees of heat or of cold; otherwise the hottest and the coldest climates, would, other things being equal, be the most salubrious. Besides the consideration

that cold is not a positive quality ; but the mere diminution of heat.

Respecting the seasons of epidemics, the statements of physicians have differed according to their particular positions. Thus, Raymond[1] says, that at Marseilles, the plague occurs less frequently at the two solstices, and Desmollins[2], that it exercises its principal ravages in the greatest heats of summer. At Leghorn, according to Giovanelli[3], summer and the first months of autumn are most to be dreaded. At Trieste, according to Verdoni[4], spring is the principa season. They[5], who resided at Malta, says that warm moist seasons *contribute* to the production of *all contagious diseases.*

The epidemics, which occasionally visit India, and the remote provinces of China, are of a milder character than the plague of the Levant. They are probably such as arise not so much from a state of the air, occasioned by backward cultivation as from famine, occasioned by the difficulty, in scarce seasons, of transporting supplies of provisions, with sufficient rapidity, from one part of the country to the other.

Yellow fever, although it most frequently occurs in America, and the West Indies, some-

[1], [2], [3], [4], [5] Howard on Lazarettos, p. 35.

times visits Europe, and Africa; but is scarcely known in Asia.

It has been, in all of these places, imputed to contagion. In America, and Europe, the contagion has been supposed to be imported from the West Indies; in the West Indies from Africa; and in Africa, no doubt, from some far distant country, perhaps from Europe, or the West Indies.

These, and all other epidemic diseases, are necessarily indigenous of every country, in which they occur; although not endemic of any.

Epidemic diseases, in all countries, both cultivated and uncultivated, occur in some districts, in some towns of the same district, in some quarters of the same town, in some streets of the same quarter, in some houses of the same street, in some rooms of the same house, and even in some corners of the same room, more frequently than in others. Of the districts, the worst cultivated, the most woody, the most exposed to particular winds, to scarcity, to inundation, &c.; of the towns, those, which are situated on the sea-coast, and are low, damp, and unprotected from certain winds; and of the streets, houses, and apartments, those, which are, in all the respects mentioned, the farthest from salubrity, and the worst built, and occupied by the poorest

inhabitants, are the most subject to visitations of disease.

These are the real causes of the local preferences, which pestilential maladies are known to manifest; and which have been so often, in vain, attempted to be explained under the doctrine of contagion. From the particular liability of sea-port towns to those maladies, and of the lowest, most crowded, and worst built streets, in those towns, the prejudice in favour of contagion, as *imported* from other countries, has been naturally strengthened. But, if it be an undoubted fact, that epidemic diseases appear most frequently in towns so situated, we also know that they sometimes affect the inland towns, without appearing in those on the sea-coast. In Medina Sidonia, for instance, which is thirty miles in the interior of Spain, the plague broke out, when no sea-port town in the neighbourhood was affected [1].

The partial effect of the winds, which is so observable at sea, is no less operative ashore. It is no uncommon occurrence, in some parts of the world, to see a ship sailing in one direction, with a fair wind, whilst another, at no great distance, is proceeding in an opposite direction, with a wind equally favourable. The locality of these operations of the elements, if I may so speak, is

[1] Burnet, p. 489. Sir James Fellowes.

exemplified in gusts and sudden changes of wind, and in the limited and uncertain course of gales, hurricanes, tornadoes, or tempests. Let us suppose two local currents of wind to meet in the neighbourhood of a long morass, dividing it into two equal parts; the persons to leeward, *i. e.* on the opposite sides of the opposite ends, would be affected with disease; whilst those to windward, *i. e.* at the reverse opposite sides of the opposite ends, and those at both ends, would remain exempt. The effects of an interchange of visits, would, in such case, be very different. The sick to leeward, by visiting their friends to windward, or laterally, would recover; and none of the persons to windward would be affected, by the presence, or contact, of their sick neighbours. But, if the windward or lateral inhabitants were to visit their friends to leeward, they would become affected with disease, through the means of the air, whilst it would be supposed, under the existing belief, to be communicated to them by contact with the persons of their sick neighbours.

There are, in all towns, places more insalubrious than others; and, upon whatever causes this unhealthiness depends, it is certain that, to inhabit them becomes the lot of those who can least afford to pay for situation. One reason, amongst many others, which gives the Mahommedan a greater exemption from plague, than the other

inhabitants of the Levant, is their ability to possess themselves of the most elevated and healthy situations, in each town; as is the case in Smyrna, and the suburbs of Constantinople. And hence, from this circumstance alone, the quarters inhabited by the poor, would, *cæteris paribus*, be always the most liable to epidemic diseases. The plagues which affected London, in 1626, and 1636, according to L'Estrange, broke out in Whitechapel. In 1665, it made its first appearance, and produced its principal mortality in St. Giles's [1]. That of Marseilles, in 1720, appeared first, continued longest, and proved most fatal, in the *Rue de Lescalle* [2]. The late epidemics of Cadiz, appeared first in the Santa Maria quarter [3], those of Gibraltar, in Boyd's Buildings [4].

If this be the case, with respect to the poor, who are endowed with the convenience of a fixed habitation, what must be the liability of those unfortunate beings, of whom there are so many in every large town, who are under the necessity of seeking a temporary asylum, from night to night, from the inclemency of the weather, under a shed, or a portico, or perhaps at the corner of a

[1] History of the Plague, by H. F. passim.
[2] Journal of the Plague of Marseilles.
[3] Sir James Fellowes's passim.
[4] Burnet, Sir James Fellowes, &c.

street? Of all the poor, these are generally the first victims.

In the Levant, servants are esteemed more liable to epidemic diseases than their masters and mistresses. This Howard, according to his notions, imputes to their being more frequently in the way of infection, by going of errands, and to market[r]. The fact seems to be correct. At Bucharest, Mr. Fleischackle, the Austrian agent, a respectable and intelligent gentleman, informed me, that he had known several instances, in that town, of servants being attacked with the plague, whilst the heads of families had remained exempt from the malady. It is usual for the servants there to sleep upon the ground floors, which are, for the most part, damp and unwholesome; whilst the apartments occupied by the family are more dry and lofty: and this Mr. F., although a believer in contagion, admitted to be a probable cause of aggravation of the malady. There can be no doubt, according to what I consider to be the real cause of epidemic diseases, that going to market, amidst putrid vegetable exhalation, and the offals of animals, together with exposure, in passing and repassing, to the noxious vapours from a marshy soil, or the effluvia from dirty lanes and ditches, or those sharp currents of air,

[r] Lazarettos, p. 25.

which are to be occasionally met with at the corners of streets and other places, and excessive exercise, or great vicissitudes of temperature, in the course of these excursions, will frequently render persons going abroad more liable to disease, than they would have been by remaining at home, during a season of pestilence. And these observations may serve to account for the facts, that servants are more liable to epidemics than their employers; as well as that shutting themselves up in their houses, if they are in airy situations, may tend to preserve persons from attacks of pestilence.

Accordingly, we find that persons, who are exposed to considerable vicissitudes of temperature, as bakers, blacksmiths, cooks, &c. are regarded as particularly liable to plague [1].

Persons, who lead an indolent, or recluse life, and at the same time live well, are, upon similar principles, more exempt than persons of a different description from attacks of epidemic maladies. This Julius Cæsar Kelli believes to be peculiarly the case, with respect to Imauns, and Armenian priests; and the exemption, taking contagion for granted, he whimsically enough attributes either to their courage, or to their perfumes [2]!

That excessive labour, and all sorts of violent,

[1] Wittman, p. 531. [2] Unpublished Manuscript, &c.

or irregular exertion, whether of body or mind, render the persons exposed to them the more liable to epidemic diseases, as well as to other maladies, is quite obvious, and cannot require explanation.

Persons in a state of inanition, from a scarcity of subsistence, whether that be real, as arising from an actual dearth of provisions, or the inability to purchase or procure them; or artificial, as from monopoly, or the voluntary abstinence, which persons sometimes impose upon themselves, from religious motives, as a penance, are, of course, greatly more liable to epidemic diseases, than persons of an opposite description. From the former cause, a great proportion of the mortality, which occurred in London, in 1665, appears to have happened. The supply of London was rendered scarce and precarious by the terror inspired in consequence of the apprehension of contagion; and multitudes were unable to purchase provisions, from having, in consequence of the same terror, been thrown out of employ, and others unable to procure them, from the fear of approaching, or being approached by, any supposed source of infection. Of the hundred thousand persons, who died of the plague that year, (for this was considered to be the real number, although the bills of mortality gave less), between thirty and forty thousand are believed to have

consisted of persons, most of them servants, thrown out of employ, by the circumstances of the pestilence [1]. The same thing must necessarily happen in all places, affected with epidemic diseases, where the belief in contagion prevails. And, if, in Turkey, that belief had chanced to operate, so as to affect the supply of provisions, to throw people out of employ, to deprive them of attendance, to banish them from society, and to place them in solitary confinement, the mortality at present arising from plague in that country, would be increased more than four-fold. To a certain degree, indeed, these causes, although they are not recognised by the Turkish municipal regulations, take place amongst the Frank [2], Greek, Armenian, and Jewish population, throughout the Othoman empire; producing, in that ratio, a mortality greater than amongst the Turkish inhabitants.

According to the best information I have been able to obtain, the days of fasting among the Catholics of the Levant, including Greeks and Armenians, are upwards of two hundred in the year [3]. On this subject, as it respects the Ar-

[1] History of the Plague, by H. F. pp. 114—116.

[2] Frank is the term applied in the Levant indiscriminately to all persons wearing the long dress, or costume, of the modern nations of Europe.

[3] The number as stated in our Calendar amounts to 127.

menians, a celebrated female writer says: "The chief precepts of their priests enjoin the strict keeping of their Lents, which are at least seven months in every year, and are not to be dispensed with on the most urgent necessity; no occasion whatever can excuse them, if they touch any thing more than mere herbs or roots (without oil) and plain dry bread. This is their constant diet. Mr. W——y has one of his interpreters of this nation, and the poor fellow was brought so low by the severity of his fasts, that his life was despaired of. Yet neither his master's commands, nor the doctor's entreaties, (who declared nothing else could save his life) were powerful enough to prevail upon him to take two or three teaspoonfuls of broth[1]."

Howard remarks, that, in the Levant, Catholics are, during Lent, more liable to the plague, than Protestants; and that Europeans, in general, are less liable to it than Greeks, and particularly Jews[2]. The Catholics of the Levant, as the Greeks and Armenians, have more fasting days than those of Europe; and some of their periods correspond with those of the ordinary plague season. In this respect, the Mahommedans have a great advantage over every other population of

[1] Lady M. W. Montague's Letters; Letter XLII. to the Countess of ———.

[2] Lazarettos.

the Levant, excepting the Protestants, who are very few in number. The Rhamadan, or Lent of the Turks, only lasts during one month of every year; and, as it observes an annual progression in its periods, it seldom coincides with those of the ordinary pestilential season. Besides, if the Turks fast, during their Lent, from sun-rise to sun-set, they make ample amends for this privation, from sun-set to sun-rise.

According to this view of the subject, we may assume the following scale of exemption, as bordering on correctness. The Mahommedans may be regarded as exempt from plague in the first degree, by reason of their having scarcely any fasts, of their inhabiting the most elevated and healthy situations, and of their entertaining no apprehensions of contagion. The Protestants may be placed next in rank, if not upon a par with them; since, although they entertain a considerable dread of contagion, they almost, if they do not wholly compensate for this, by their more appropriate mode of living, and other circumstances[1]. The Catholics are undoubtedly the

[1] Of Christians, the British inhabitants of the Levant may perhaps be regarded the most exempt from this calamity, which I would impute to their living at all times in a manner calculated for a season of pestilence. Of two British subjects, who some years ago had the plague at Con-

most subject to this malady, both on account of their extravagant fastings, and their no less extravagant dread of contagion. And the Jews may be considered the next most liable to it, from their participating in the dread of contagion, from their generally inhabiting the least salubrious quarters of cities, and from circumstances connected with their habits and manner of living.

Strangers to a climate, other things being equal, are universally allowed to be more liable to the epidemic diseases, which occur in it, than natives, or persons who have made a long residence: " En general les enfans et les jeunes

stantinople, one, Mr. Cartwright, (now Consul at Patras) recovered, and the other, Mr. Maclachlin, perished. They were living together, and their sickness happened at the same time. From the account which I received from Mr. Cartwright of both cases, I am persuaded that, by a very moderate degree of medical aid, the one might have survived, and the other recovered much sooner from his malady. By themselves, and by others, their disease was, of course, imputed to contagion. From an attentive consideration of the circumstances, which were communicated to me in a very satisfactory manner, although only verbally, I have been led to conclude that the cause of their malady was exposure to heat, moisture, and fatigue, whilst shooting quails, immediately preceding the attack. We know, that, in the West Indies, persons are frequently seized with yellow fever, in consequence of exposure to the sun, and excessive exercise, in the field; and, in the East Indies, by exposure of a similar kind, to Coup de Soleil, and Hepatitis.

gens sont plus exposé à cé fléau que les personnes d'un certain àge: et des observations constantes nous prouves que partout, mais particulièrement dans la capitale, les etrangers, les voyageurs, et tout ceux qui n'y sont domiciliés que depuis peu, en sont encore plus susceptibles que les naturels du pays [1]." In corroboration of these remarks, I may observe, that the patients brought to the pest hospital, near the Seven Towers, at Constantinople, during my residence there, were persons from the provinces, or recently arrived in the capital. And we are told, that, during the plague of London, in 1665, of 3000 that fell sick the first week in November, the greatest number were new comers [2]. The great liability of persons arriving in the East and West Indies, from other countries, to such fevers as are incidental to those climates, is proverbial. "The yellow fever," says Dr. Mac Arthur, "is almost invariably confined to men recently arrived in the country [3]."

It is a well known fact, that aged and infirm persons, women and children, are less liable to epidemic diseases, than the young, vigorous, and male part of a community. In the plague of

[1] D'Ohsson, II. §. vi.

[2] History of the Plague, by H. F. p. 263.

[3] On the diseases of Barbadoes, Med. Obs. Vol. VII. p. 326.

Cyprus, in 1760, ten men are said to have died to one woman; but it was almost universally fatal to the young of both sexes. Many places were left so destitute of inhabitants, as not to have enough left to gather the fruits of the earth [1]. It ceased in July. This statement may be exaggerated; but, it shews at least that the distinction is not without a foundation.

Dr. Tadeo Lafuente, in his short description of the yellow fever, with which Medina Sidonia was afflicted in 1801, from the middle of August to the commencement of November, has this remarkable observation: "It attacks every individual in the different houses, *except the children at the breast* [2]."

Physicians, nurses, and attendants upon the sick, do not appear to be peculiarly liable to the attacks of an epidemic; nor persons, who are employed in lazarettos to expurgate goods. On the contrary, there is reason to believe, that they are proportionally even less so, than other classes of society.

The description of persons alledged to be most exempt from plague, tends strongly to confirm this doctrine. Boyle has observed that those, who dwell near quicksilver mines, are more rarely infested with plague. The merchants of Cairo affirm that oil sellers, water-

[1] Philosophical Transactions for 1763, No. 12.

[2] Burnet, p. 493.

carriers, and tanners, are little liable to the malady [1]. And a tradition prevails in the neighbourhood of London, that no person, who had taken refuge near the tan works, at Bermondsey, was affected with the plague, in 1665.

Mr. Julius Cæsar Kelli affirms, that no tanner of leather, or Bassemadje, has ever been known to be attacked with the plague, owing, says he, to the drugs, and oils, which they use in their shops [2].

But, if epidemic diseases depended upon contagion, these things could not possibly happen. Contagion being applied, disease would as readily seize the rich as the poor; the well-fed, as the ill-fed; the well-clothed, as the ill-clothed; the well-lodged, as the ill-lodged; the idle, as the laborious; those who dwell in a pure, as those who dwell in an impure atmosphere; the Protestant as readily as the Catholic; the Hindû as the Christian; the Mahommedan as the Jew. If the virus of small-pox were applied to a great many persons who had not previously had the disease, the number of those who might escape the malady, would be extremely small. But, the the number of those, who escape, in epidemic seasons, is very various, generally, however, greatly exceeding the number of those that are seized.

[1] Kelli's Manus. Observations, &c.

[2] Wittman, p. 531.

It is also obvious, that, if such were the cause of epidemic diseases, not only those having communication with the sick, during a pestilence, could not, but in a very insignificant proportion, wholly escape infection; but, as these diseases are capable of affecting the same person repeatedly, no person, who was exposed to such communication, could escape with only one attack. All within the infectious distance, whatever that might be, would again and again be seized. Physicians, nurses, and attendants upon the sick, quarantines and lazarettos especially, would be as so many nuclea, from which, as from common centres, the infection would perpetually diverge, to every point of the circumference, of a ship, a house, an hospital, a camp, a city, a province, or a state. And, if it were capable of being conveyed by goods, it could not be restrained even within the limits of individual nations. It would, in that case, never cease until the whole human race was extinguished.

## CHAP. XI.

*The origin of alledged contagions in particular epidemics has been a subject of endless controversy—the doctrine of contagion, and its importation from foreign countries, favoured by the operation of self-love, policy, &c.*

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In the preceding chapters it has been shewn in what manner the simple and natural doctrine of the ancient physicians, that epidemic diseases depend upon the air, as well as the popular superstitions, that they depend upon the anger of the gods, and upon various supernatural causes, have been supplanted by the grosser superstition, that they depend upon contagion.

Besides the weight of the authority, by which it was at first introduced, there are principles deeply rooted in human nature, which have tended to confirm the belief in this doctrine. These are self-love, avarice, and ambition.

To impute a fatal malady to a domestic, or personal origin, would be to utter a severe reproach. It is reckoned disgraceful to be thought capable of communicating even the itch. Hence,

to consider a pestilence indigenous, or endemic, is repugnant to the self-complacency of nations; and to import them from distant countries, by any means, (and a specific contagion, which was chosen as the Catholic medium of their conveyance, was as good as any other,) is consoling to self-love.

This principle, which is so strongly operative in the human mind, impelling us to seek the origin of every evil any where rather than with ourselves, has been, no where, more active than in tracing to a foreign origin the source of epidemic and pestilential maladies. It is this principle, which has led the Americans (contagion being always taken for granted, and no proof of its existence required) to trace the epidemics, which have desolated their country, to the West Indies; the inhabitants of the West Indies to Africa; those of Italy and Spain to America, or the West Indies, or to one another; the people of every country, to some neighbouring, or distant state; the English to Holland, and the Dutch to England; the Muscovites to China, and the Chinese, no doubt, to Muscovy.

It is also considered political, as tending to prevent, or to dissipate popular alarm, to give to a severe disease, supposed to be propagated by contagion, a foreign origin.

Avarice and ambition powerfully concur to support this policy. I have been informed that many persons in America strenuously opposed the opinion, that the epidemics of that country were of domestic origin, lest the belief should diminish the value of lands and houses; or retard the prosperity of the country, by discouraging emigration.

The idea of an indigenous malady is also supposed to operate unfavourably upon the commercial relations of countries: and this consideration is reported to have had some share in dictating certain opinions, which were published, respecting the cause of the fever at Leghorn, in 1804.

It is much to be regretted, when members of an enlightened profession degrade themselves by becoming the instruments of a temporising, time-serving, shallow, and contemptible policy, which cannot compensate, by the highest possible sum of local or temporal advantages, were they to accrue, the great evils, which it must ultimately occasion, by the retardation of the progress of scientific improvement.

One of the most extraordinary attempts to assign a distant origin to an assumed contagion, in any epidemic, is that which has been made by Mr. Pym, respecting the fever of Gibraltar[1]. It will be

[1] Observations upon the *Bulam* fever, which has of late years prevailed in the *West Indies, on the coast of America, &c.*

recollected, that, in 1793, an epidemic prevailed, in Grenada, in the West Indies, the contagion of which Dr. Chisholm imagined he had traced to importation from Bulam, on the coast of Africa. This identical African contagion, after many vicissitudes, and travels through various parts of the globe, and having been repeatedly annihilated, and as often undergone a resuscitation, at length makes its appearance on the shores of the Mediterranean. From thence it has, by the newspapers, been occasionally re-exported to the West Indies, and to America: from whence it is by no means impossible, that we shall hear of its returning, in the course of revolving peregrinations, to revisit its native soil. Let us take a survey of the ingenious, not to say scientific, process, by which the fevers of Bulam, Grenada, and Gibraltar, have been thus identified. When fevers of a similar description have occurred simultaneously, or at different periods, at those places, it has been gratuitously assumed that they could not be the result of like causes simultaneously, or at different periods, operating in all of them; but, that they must have been propagated by means of a specific contagion

*at Gibraltar, Cadiz,* and *other parts of Spain,* &c. by W. Pym, Esq. Deputy Inspector of Hospitals, late Superintendant of Quarantine, and President of the Board of Health, at Malta, 1815.

from the one to the other. At the period of its appearance at Grenada, in 1793, a vessel most opportunely arrived there, from Bulam, on the coast of Africa; and the disease being first assumed to depend upon contagion, it was at once concluded that the cause of the calamity had been imported in the vessel (the Hankey) that had just arrived. So far Dr. Chisholm's: now for Mr. Pym's assumptions. He assumes that the fever, which left the coast of Africa in 1793, after having performed sundry voyages and travels, had been regularly or irregularly imported and exported from various countries, alternately by bales of goods, by soldiers, by seamen, or by smugglers, had at length contrived to elude the vigilance of the quarantine and plague police establishments, and, in 1804, to make a tenable lodgment in the garrison of Gibraltar: from whence it has carried on a desultory warfare, almost annually entering, and being expelled, from some of the strongest police fortresses of the Mediterranean [1].

[1] The London Daily Journals of the 27th of December, 1816, have re-exported this fever, with some change of colour, from the Mediterranean to the West Indies: "The New York papers to the 25th of November," say they, "contain a letter from St. Bartholomew's, dated the 29th of October, stating that a dreadful fever rages at Guadaloupe, of which great numbers of people are dying every day. The fever is *reported* to have been brought by a vessel from Malta, and is called *the black fever*."

Let us put the assertions of Mr. Pym in the shape of syllogisms. 1. The fever, which appeared at Grenada, resembled, in its symptoms, that, which had prevailed at Bulam: the fever, which appeared at Gibraltar, resembled, in its symptoms, that, which had prevailed at Grenada: *Ergo*, the fever, which appeared at Gibraltar, on the coast of Spain, in 1804, must have been the identical fever, which had disappeared at Bulam, on the coast of Africa, in 1793; Q. E. D.

2. Those persons, who had the fever at Bulam, had it not at Grenada; and those, who had it at Grenada, had it not at Gibraltar: but all diseases, which are incapable of affecting the same person repeatedly, depend upon contagion: *Ergo*, the Gibraltar fever, which is the Grenada fever, which is the Bulam fever, must have depended upon contagion; Q. E. D.

I do not pretend to use Mr. Pym's words, or his precise forms of probation. But his meaning, at least, is not burlesqued. To encounter with serious argument, assertions, which can be better refuted by ridicule, would be a superfluous exertion of the powers of reasoning. "Ridiculum acri fortius ac melius plerumque secat res."

We are not informed by what routes, or stages, the contagion travelled from Bulam to Gibraltar: but as it took 11 years to perform

the journey, we may conclude that it touched at many ports and places, and did much business, on its way.

If the circumstances of this narrative had been reversed [1], and a ship had, during the existence of the epidemic, arrived at Bulam from Grenada, or at Grenada from Gibraltar, the malady, if the physicians of Bulam could make nosological distinctions, like their more learned brethren of Europe, would undoubtedly have been named after the peninsular fortress. Query, Ought the fever, reported to have been last year imported into Guadaloupe from Malta, to be called the Malta fever, the Mediterranean fever, or the Gibraltar fever; or ought it to be regarded as the old Bulam fever, emerging, with renovated vigour, from the bosom of the Atlantic ocean?

Whenever a pestilential disease occurs, contagion being of course assumed as the cause, and

[1] A copious narrative of the transactions of the ship Hankey, and an account of the alledged contagion in the Grenada fever, together with a superfluous refutation of it, may be found in Dr. Bancroft's Essay, p. 688—768. If the refutation of the alledged contagious properties of any particular epidemic inferred the entire refutation of that hypothesis, then such disquisition might be of use: but, as, on the contrary, any refutation, given, not as an example, but as an exception to the rule, involves the virtual acknowledgment of the doctrine of contagion, generally, it can be considered, if that doctrine be erroneous, as only misleading.

the means of cure being unknown, or impossible to be applied, all that remains to be done is to trace the source of the contagion so assumed, and to adopt corresponding means of prevention. The latter consist of course of all the usual modes of separation, seclusion, and restriction, enjoined in all regulations of plague police : and, as to the former, seeing that thousands of sources, all equally probable, are constantly within the reach of the imagination, the enquirer need never be at a loss. Thus the contagion of typhus, for instance, may be generated in, and conveyed by persons, clothes, or goods, from alleys, jails, hospitals, or ships ; that of yellow fever, may be generated in Spain, America, the West Indies, or on the coast of Africa, and transmitted to all other places, by means of the produce of these countries respectively ; and that of plague, deemed exclusively a native of the Levant, may be conveyed, directly or indirectly, in so many ways, by the medium of the goods, wares, and merchandises of that part of the world, enumerated as susceptible of contagion in the first and second degrees, to all the other quarters of the globe, with which they do, or do not hold intercourse, that he must be very deficient in ingenuity indeed, who cannot, upon the appearance of any epidemic, discover at once some assignable source of contagion.

This extraordinary facility of tracing effects to their causes, brings to my recollection a prejudice somewhat similar, of which instances must be known to almost every reader. There is a legend that any unusual mark, with which a child is born, is produced by the imagination of the mother, in consequence of her having longed, during pregnancy, for some particular object of food, &c. with which she was not gratified. No sooner is the mark announced, by the observant midwife, than the mother sets about recollecting, with the help of the good women around, the various articles, for which she might have in vain wished, during the period of gestation, as grapes, strawberries, or other fruit, venison, or turtle, out of season, rare fish, or curious wines, &c. &c. &c. She seldom fails to recollect some object bearing a resemblance sufficient for her purpose to the mark upon the child; or to recollect, that, at the moment of longing, she placed her hand, or finger, according to circumstances, upon that part of her own body, which corresponds with that which is marked in her offspring.

Here we have cause and effect traced to the entire satisfaction of the parties. But, when no distinct recollection remains of the particular process by which the discolouration on the infant's skin was produced, still no doubt is enter-

tained that it was the result of the power of the mother's imagination, in some manner exercised.

So, when an epidemic disease occurs, it is immediately concluded, that it depends upon contagion; and the only question to be decided is, how this contagion arose, or whence it had been derived. It is seldom that no plausible origin can be assigned to it; and, when this happens to be the case, the conclusion notwithstanding remains the same.

These prejudices have a farther resemblance. The objects, supposed capable, through the imagination of the mother, of having their image impressed upon the skin of her unborn child, are of capricious selection, and may be multiplied ad infinitum, like those objects of merchandise, from the Levant, which are susceptible, in the first and second degree, of receiving, retaining, and communicating the contagion of plague.

But here the similitude ends: for whilst the one prejudice is only absurd, or ridiculous, the other is highly pernicious in its operation.

Having stated these preliminaries, I shall now advert to some of the histories, with which we have been amused, respecting the origin of the contagion, to which different epidemics have been imputed.

As much has been written, concerning the epi-

demic diseases, which have, of late years, afflicted Spain; and, as they excite an interest proportionate to the proximity of the periods of their recurrence, and the theatre of their devastations, respectively, I shall here give to the origins, which have been imputed to them, a priority of consideration.

If we have but little authentic information, respecting epidemic diseases, in Spain, or elsewhere, previous to the fourteenth century, it does not, by any means, follow, that they have not equally occurred: and, if we find that similar laws apply uniformly to those, which have succeeded, there can be no good reason why we should not infer that they are equally applicable to those which have preceded, that particular period, at which our information respecting them begins.

The first plague in Spain, of which we have any account, commenced in 1348, and continued for three years; by which, says father Samiento [1], two-thirds of the population of the country were destroyed.

The first great sickness recorded to have happened in Cadiz, is in 1466, and to have nearly depopulated the city [2].

[1] Padre Samiento in su dictamen sobre la mesta.

[2] Ferreras, Synopsis Historica Chronologica de Espāna, parte decima, p. 199.

In 1507, a fatal malady prevailed in that city, and in Barcelona [1]. These disorders were all supposed to be the true plague.

A sickness again afflicted Cadiz, in 1582, of which nothing satisfactory is related. It was believed to have ceased through the intervention of St. Roque, to whom the city, in consequence, dedicated a chapel, upon the spot where the barracks, which bear that name, are now situated [2].

Hitherto we hear nothing of contagion. But it is extremely probable that the doctrine prevailed at this latter period, since it had been promulgated from the Vatican thirty-five years before. In all the succeeding epidemics of Spain, the belief in contagion has invariably prevailed.

In 1600, and the two following years, Malaga was afflicted with a pestilence, which carried off half the population [3].

In the year 1637, a similar calamity destroyed 40,000 persons in two months [4]. These, we may presume, were two of the autumnal months.

[1] Capmani, Compendio historico y Cronologico de las pestes, contagioz y epidemias, tom. 4. de las memorias historicas, No. 7, p. 66.

[2] Sir James Fellowes, p. 22.

[3] Ibid. p. 152.

[4] Ibid.

In 1648, 49, and 50, a disorder prevailed, which was also very fatal to the inhabitants [1].

This epidemic also appeared at Cadiz, where it lasted three years, carrying off more than 14,000 persons; and at Seville. It "was *introduced* into Seville by a vessel that anchored in St. Lucar de Barrameda, with merchandise from the Levant; the *germs* of the disorder were *conveyed* to Malaga and Murcia in silk stuffs taken from the same ship, and it spread afterwards to Cordoba, Eciga, Valencia, and its neighbourhood: in the course of two months and a half, 46,000 persons were carried off in Seville, and in the adjacent towns [2].

In 1649, the plague also raged at Gibraltar, and was imputed to the same origin [3].

In 1678, there was a pestilence at Malaga [4].

[1] Sir James Fellowes, p. 159.

[2] Reports, p. 23, and note. The montas of mortality, I think myself justified in inferring, were two of the autumnal months. Don Gaspar de Heredia wrote an account of this plague in Spain, and Don Alonzo de Burgoz, physician to the Inquisition at Cordoba, published a work on the subject, entitled, "Tratado de la Peste, su esencia, preservacion y curacion, con observaciones muy particulares." Cordoba, por Andres Carillo ano 1651, 8vo. Don Nicolas de Vargas also wrote an account of this plague.

[3] Ibid. p. 81.

[4] Ibid. p. 159.

In 1681, a fever appeared at Cadiz: "and, upon this occasion, we are told it was in contemplation to send to Marseilles for a person celebrated for his fumigations against the plague[1]."

In 1727, a great sickness and mortality are said to have taken place in the garrison of Gibraltar, after the siege which ended on the 11th of June in that year: "The garrison of Gibraltar lost very few men in the course of the siege, and fewer officers; but soon after, the soldiers, through excess of drinking, and want of exercise, died in vast numbers, for it is computed that 500 were buried in three months[2]." As these were the autumnal months, we are warranted in inferring, that, whatever share of the sickness and mortality may properly be attributed to excess in drinking, and want of exercise, much must have depended upon the state of the atmosphere, at the usual epidemic season.

In 1730, a fever accompanied by the symptom called "El Vomito Negro," or black vomit, made its appearance, it is said, for the first time, at Cadiz, and destroyed great numbers of persons, "very few," says Sir James Fellowes, "having escaped, who were attacked with the

[1] Ibid. p. 23. Cadiz ilustrada, lib. 6. cap. 19. p. 483. y siquientes.

[2] Colonel James's History of the Herculanean Straits.

disease [1]." It prevailed the following year at Cadiz, with livid, yellow, or dark spots, and black vomit, and in other parts of the Peninsula; and spread great terror throughout the country. Don Francisco Fernandes Navarrette, Professor of the University of Grenada, and Physician to Philip V. affirmed, that it was introduced into Cadiz, by a vessel from Spanish America [2].

The disorder, with this symptom, appeared in Malaga, for the first time, as is said, in 1741; when it carried off upwards of 10,000 persons. To this epidemic a South American origin was also assigned [3].

[1] Reports, p. 23. The year 1730 was remarkable for the drought that was supposed to have occasioned the influenza, which prevailed very generally at that time; some barometical observations shewing the increased weight of the atmosphere, during the period, made by Gottlibius Ephraim, in his treatise "de efficaciâ aëris in corpore humano" correspond with those made by Fernandez Navarrette in Castile and Andalusia.

This author wrote to the Spanish physicians, exhorting them to give a history of the caterrhal constitution (which returned in 1738), to investigate its origin and causes, and to point out such measures as might tend to prevent the disorder (la peste) which then threatened all Spain. This work (8vo.) was dedicated to the Royal Academy of History at Madrid, in 1738. p. 25. note.

[2] Ibid. pp. 24, 25.

[3] Ibid. p. 160. The Conversaciones Malaguenas, refers to the works written upon this subject, entitled, "Crisis

With respect to these phœnomena, they do not indicate a new disease, being in effect nothing more than symptoms of a higher affection; which must always have appeared, whenever the pestilential atmosphere has manifested equally noxious properties. Their having been rarely, if at all adverted to, only shews that the degree of noxiousness, capable of producing them, has rarely, if at all, occurred, excepting at the periods stated. The black vomit indicates a state of the blood, approaching to dissolution, or disorganisation, of that vital fluid, which, in any considerable degree, must be incompatible with life, and does not often take place in the more temperate climates. But, if its occurrence has not been related of any of the fevers of former periods in Spain, it cannot be that the disease has not occasionally arisen to such severity as to exhibit this symptom, but that no historical records of epidemics were then kept.

The next epidemic, of which we read, in Spain, is that of 1764, which, it appears, was solely confined to Cadiz. Salvaresa supposes it

epidemica que le padeció en Malaga, Año 1741, Por Don Nicolas Francisco Roxano."—"Analisis Medica de la Epidemia que se padició en Malaga, Por Don Antonio Rubio," and "Sinopsis e Medica Sobre la Epidemia de Malaga, Por Don Francisco Reyes Sahagun."

to have been occasioned by the old and corrupted corn. "Amongst the poor, whose diet consisted chiefly of bread, the disorder was most violent. In this year the animals were first affected, and the mortality was particularly observed amongst those which fed on grain; viz. poultry, pigeons, &c. Insects, called by the Spaniards *langostas*, were also seen there previous to the breaking out of the fever. The same kind of insect made its appearance in the spring of 1800. The domestic animals were not affected, until the disorder became general [1]."

I may here observe, that, however bad corn, or deficient nourishment of any kind, may aggravate, or be capable of producing an epidemic disease, the qualities of the air must be presumed to have had the principal share in producing the fever of Cadiz, in 1764, since it happened at the usual epidemic season, viz. "in the months of September and October [2]." Indeed we may infer, with much probability, that this fever would have occurred, with very little less severity, had the corn been as wholesome as usual; and that it would not have occurred, in the same degree, at any other season of the year, had the corn been of a quality still worse.

[1] Reports, p. 27, 8, Note .

[2] Essay on Diseases incidental to Europeans in hot Climates: by James Lind, M.D. &c. Part I. c. iv. p. 122.

In an account of the fevers which have prevailed in Catalonia, at different periods, from 1764 to 1783, Don Joseph Masdeval, considers them of course contagious, and ascribes the origin of the contagion, in 1764, to the passage of French troops, in alliance with Spain, proceeding from Portugal through Catalonia [1]. Was not this also at the usual epidemic season?

Colonel Drinkwater gives an account of an epidemic, called an Influenza, which made its appearance at Gibraltar, in August 1782, "which was attributed, at that time, to the extraordinary heat of the atmosphere, owing to the prodigious fires made by the Spaniards, on the neighbouring hills, but was afterwards ascertained to have been general throughout Europe [2]."

We now come down to the commencement of the present century, a period celebrated in the history of epidemic diseases, in Spain. But I may observe, that, previous to this period, the origin of almost every epidemic disease, which has occurred in that kingdom, and particularly of those which have afflicted Malaga, has, probably for no other reason than the constant communication between the two countries, been re-

[1] Relacion de las epidemias de Calenturas putridas y malignas. Madrid, 1797, Por Don Joseph Masdeval.

[2] Siege of Gibraltar, p. 257.

ferred to South America, which is certainly not remarkable as a seat of pestilence [1].

Respecting the origin of the supposed contagion of the fever of Cadiz, in 1800, Sir James Fellowes has the following remarks: "It was *rumoured* that a vessel, called the Dolphin, had arrived from Spanish America, in which were the *seeds* of the disorder, and that some of the smugglers, who had been frequently on board, during her quarantine in the bay, were the first persons taken ill, in the streets of Boquette and Sopranis, in which they resided, and where it is *supposed* they had secreted their *goods* [2]."

Instead of having had recourse to this curious, and certainly not very scientific apparatus, of *rumours*, and *suppositions*, and *seeds* of contagion, *imported in goods*, from South America, in which it has not even been shewn, that any pestilential disease, did, at the time, prevail, and introducing this new species of contraband, by means of smugglers, into the streets of Boquette

[1] Upon all these several occasions (of epidemics at Malaga) "The *contagion*," says Dr. Mendoza, "was introduced by vessels arriving from South America." *Hist. de las Epidemias padecidas en Malaga, en los años de* 1803 *y* 1804. *Compuesto por el Medico, Don Josef Mendoza, en Malaga*, 1813.

[2] Reports, p. 37.

and Sopranis, would it not have been a more rational conclusion, unless it were determined at any rate that no *other* power than contagion *should* be the cause of that disease, that it might have depended upon the state of the air in those unhealthy parts of the town, in which it had first appeared, and upon other circumstances connected with the situation of their inhabitants, rather than upon the accidents of the persons first seized having been, by profession, smugglers, and having, in the exercise of their vocation, been on board of a vessel recently arrived from America?

Sir James Fellowes relates a conference, which he afterwards had, with the Spanish Captain of the Dolphin, with a view to ascertain the correctness of this rumour [1]: from the result of which it does not appear that there were any circumstances, connected with the arrival of that vessel, in Cadiz, that could have justified the imputation of her having been the medium of propagating the infection, were it even admitted that an epidemic disease could ever depend upon contagion, or that the epidemic in question had not prevailed, at the usual season of pestilence, in that place.

The fever, of 1804, was ascribed, by some

[1] Reports, p. 437.

Spanish physicians, to Moorish origin [1]. But for this there appears to be no better ground, than, that, in the years 1799, and 1800, a plague had broken out, at the usual pestilential season, on the northern coast of Africa, "and spread such devastation amongst the Barbary States, that, at Fez, many thousand inhabitants fell victims to its fury, and the towns of Tetuan and Tangier, which had always before this period kept up a constant communication with Gibraltar, were nearly depopulated. In Tangier alone upwards of 2000 Moors had been carried off, before the end of July, 1800 [2]."

It would have seemed strange that it should have been the epidemic of 1804, at Cadiz, and not that of 1800, that was attributed to Moorish origin, seeing that the smugglers between the Coast of Africa and Spain, were not less active in that, than in subsequent years, did we not know that the grounds usually assigned for these origins are entirely matters of fancy and caprice. General O'Hara, at that time Governor of Gibraltar, writes to the Duke of Portland, then Secretary of State, dated August 10th, 1800, "in consequence of information he had received that some smugglers from Gibraltar, who had landed their tobacco in Spain, had been pursued by the Spa-

[1] Reports, p. 94. [2] Id. p. 92.

nish armed boats, and disembarked in Barbary, where the plague then raged, and that, on their return, denying any communication with that country, were admitted to pratique and came into the garrison; they *were seized and put into a Lazaretto under rigorous quarantine, together with the inhabitants, with whom they had communication, to the number of nineteen, and, by the advice of the faculty, the boat, in which they came over, was burnt; that* HAPPILY NO BAD CONSEQUENCES FOLLOWED," &c [1].

This happened in the month of August, at which time epidemic diseases usually occur in those latitudes; the smugglers, of whom one is stated by Mons. de Leully, a French surgeon, to have been visited by him, having the plague, had already had commmunication with the town; yet the exemption of the garrison from the disease is imputed to the burning of the boat, the burning of materials which are not supposed even in the Levant to be susceptible of contagion [2].

By other accounts, the fever of Cadiz, in 1804, was said to have originated from French ships, which had arrived in the Bay of Malaga [3]: of which, when we come to enquire into the assigned origin of the epidemic of that place, we shall see the complete absurdity.

[1] Reports, p. 94.

[2] Id.

[3] Id. p. 95.

Of the fever of 1810, Dr. Mellado [1], Physician to the Board of Health, has endeavoured to shew that it did not originate in Cadiz; but acknowleges the difficulty of tracing it to its source.

The fever of Cadiz, in 1813, in respect to its cause, was, from the peculiar circumstances of the times, treated in some measure as an object of policy, rather than of medicine [2]. Some of the politicians on both sides, were, no doubt, perfectly sincere in the opinions which they delivered. But the opinions of the advocates for contagion prevailed. Amongst these were Sir James Fellowes, and some of the British medical officers, who served under his command; and the Spanish physicians Arejula, Gonzales, and Flores [3]. Of the names of those, who maintained the opposite opinion, we are not informed, nor of the grounds of their belief. This, however, is of no manner of consequence, as we have nothing to do with the weight, or preponderance of those parties, when thrown into the opposite scales. The advocates for contagion, have, in this instance, contented themselves with assertions that they considered the disease contagious, and its propagation to be owing to remissness in the observance of quarantine, and

[1] Historia de la Epidemia padecida en Cadiz el año de 1810.

[2] Reports, p. 256—275 inclusive.

[3] Id. p. 307.

other plague police regulations, without offering any proof of the alledged fact, or assigning any source of the supposed contagion. If we consider, then, that the disease appeared to be of a similar nature, commenced, and terminated, at similar periods, and went through a similar course, with all the preceding epidemics of that place, respecting which we have any authentic information, the conclusion is inevitable that it has depended, like them, upon the constitution of the autumnal season.

Concerning the reputed source of the fever at Malaga, in 1803, it was conceived, only because it was necessary to find some foreign origin for it, that it must have been imported in some one of seven vessels, that were laying in the bay; and the suspicion fell principally upon four of them; two of which were French, one Dutch, and one American. These vessels had all entered the bay, in May, or early in June, and had, from their arrival, communication with the town; whereas the disease did not begin till late in August, the usual period, at that place, of the commencement of epidemic maladies [1].

The pestilence of 1804, which afflicted Gibraltar, and so many parts of Spain, was supposed by some, to have been that of Malaga of the pre-

[1] Reports, p. 161—165, and note.

ceding year resuscitated, which, it was thought, had only been "dormant, not extinguished [1]." It was also conjectured that after having pervaded the provinces of Spain, it was only prevented from entering Portugal, by the precautions taken by the Portuguese government to cut off all communication whatever with the kingdom of Algarve [2]."

But it was not alledged to have come to Gibraltar directly from Malaga. Its entrance into that garrison was thought to be traced in the person of an unfortunate man named Santos, who happened to be amongst the first taken ill, and to have just arrived from Cadiz, where a similar fever prevailed [3]. On this allegation, I shall only make two remarks, the progress of this epidemic being more fully treated of in another chapter. In the first place, the succession, in which different persons are attacked, with any disease, can be no proof that it is propagated by contagion. In a company of a hundred persons, engaged in drinking wine, or other liquors, ebriation will not take place in all of them at once, but in a certain succession; and no man would think of asserting that the person, who first lost the power of speech, or motion, had *infected* the rest of the company. But, there are other circumstances, which, supposing the fever of Gibraltar were admitted to be

[1] Reports, p. 97. [2] Id. p. 101. [3] Ibid.

of a contagious nature, would render it quite impossible that Santos should have been the source of the *infection*.

" On referring to the register of arrivals and sailing of vessels, which is kept at the health office at Gibraltar, it appeared that the Conception, Juan Olivarez, did arrive there on the 25th of August, after a passage of 24 hours from Cadiz, and that she afterwards sailed for Lisbon on the 5th of September [1]." Now, it can neither be supposed that this vessel could have lain at Gibraltar for eleven days, having persons ill of a pestilential malady on board, without its having been ascertained; or that Santos could have come in her from Cadiz, whilst labouring under a contagious disease, without communicating that disease to the passengers and crew.

But there is a single fact recorded of the progress of this malady, which, were there no other grounds of reasoning, would alone be sufficient evidence of its real nature.

" It was remarked as a curious fact, that the Jews, who were very numerous in Gibraltar, and who, it was thought, would have been more predisposed to disease, from their habits and modes of life, were not generally attacked by the prevailing disorder, until after the 18th of September, the day

[1] Reports, p. 116.

of atonement, in which the Hebrew nation meet together in the synagogues.

"On the 19th, four Jews were reported to have died, but it was not ascertained how long they had been ill. The feast of the tabernacles took place on the 15th or 16th of September; and it is customary at this time for the friends and acquaintance to meet at each other's houses, and to enter the synagogue. Previous to this, the Jews had cautiously kept to their own families at home, and not one of them was said to be ill of the prevailing disorder, until after the feast alluded to, when there was rejoicing, and permission given for the admission of strangers amongst them [1]."

Here we have the source of the malady distinctly indicated. There cannot indeed be a more palpable cause of disease, than the exhalations in which persons are enveloped, in frequenting, and remaining for hours, in spacious uninhabited buildings, as churches, synagogues, &c. The Jews leaving their houses, first enter the synagogue on the 15th or 16th, and on the 19th four of them die, being the first deaths reported, the interval coinciding with the period, at which, after its commencement, a pestilential malady usually proves fatal. This fact alone is so conclusive, without

[1] Reports, p. 108, 9.

any reference to the periods of the commencement and cessation of the disease, or to other facts which might be adduced in such abundance, that I should account it superfluous to say one word more upon the subject of the Gibraltar Fever of 1804. To all the epidemics of a subsequent date, which have prevailed in that garrison, differing merely in degree, the same reasoning will be found equally to apply.

Dr. Riseuno has not the smallest doubt, that the disease, which ravaged Carthagena, at different periods, was, on every one of these occasions, brought from abroad. In 1804, he affirms it to have been brought from Alicant, as he says it was brought to Cadiz and to Malaga, in a Spanish man of war. In 1810, he considers it to have been, with equal certainty, brought from Cadiz and Gibraltar, by an armed coasting vessel. In 1811, its origin had escaped his observation. In 1812, he considers it as having been imported from Mazaron [1].

The physicians of the other places were, in the mean time, equally eager, for the honour of their respective residences, in reversing the progress of the disease: and it is an undoubted fact, that, in 1810, they were at Gibraltar deriving the *contagion* of *their* fever from Carthagena, at the

[1] Burnet, p. 233.

very moment that Dr. Riseuno was tracing that of the Carthagena fever to Gibraltar [1].

This was the noted fever, which had been imported in the year 1793, from Bulam, in Africa, into Grenada, in the West Indies, and, after having been there duly concocted to suit the exigencies of an European climate, re-shipped for Gibraltar [2].

It was no doubt the same fever, such is its eagerness for foreign travel, that conveyed itself to Cadiz, in 1800, according to Sir James Fellowes, through the medium of a band of smugglers [3]!

The epidemic of Malaga, in 1803, Sir James Fellowes alledges to have been introduced by two French troop ships, the Dessaix and Union, both from Marseilles, although there was, at that time, no malady supposed to be contagious, in that city [4]!

The same writer has imported the yellow fever, which appeared at Santa Cruz, in Teneriffe, in the year 1810, from Cadiz [5]!

There is nothing in nature more vague, or more palpably absurd, than the origin to which

[1] Burnet, p. 274 and 394.

[2] Id. P. 274. Chisholm passim. [3] Reports, p. 37.

[4] Id. p. 164.

[5] Id. p. 230. This fever commenced on the 16th of October, p. 229.

different epidemics have been imputed. The plague of London, in 1665, was attributed to two Frenchmen, said to be the first, who died of that disease in Long Acre, or Drury Lane, and to have in their possession some Turkish silk, which had been imported, the preceding year, from Holland, in which the infection was supposed to reside. The first, who died in the city, was also reported to be a Frenchman, who had fled, with the disease upon him, from St. Giles's. This account, in so far as the country of the persons first affected, is concerned, may, or may not be true: but, as it cannot, in any manner, affect the question at issue, it is not worth while to take much trouble to investigate the fact. Supposing it to be true, it would not be very difficult to explain, assuming atmospheric influence to be the cause of the disease, why foreigners should be the first affected. It is an acknowledged fact, that strangers are peculiarly liable to the influence of an epidemic atmosphere. It is also obvious that the manner of living of foreigners, whose necessities might oblige them to reside in St. Giles's, could not but increase their liability to be affected by any disease, of which the ordinary cause was operating. But there are grounds for believing this statement at least problematical. Considering the usual operation of that principle, which renders men prone to throw the odium of introducing any evil, rather upon a

foreigner, than a fellow-countryman, and that the proof of the fact is, in this case, insufficient, I am led wholly to disbelieve that the two first persons, who died of the plague in St. Giles's, or the first who died of it in the city, were Frenchmen, and to impute the propagation of the report, and the ready belief in it, to the influence of this ignorant self-love. That the three first persons, amongst a hundred thousand, dying of a devastating pestilence, in the metropolis of England, should be Frenchmen, is quite as incredible to my mind, as that one leather cap, in a town of *Italy*, should successively *infect* twenty-two persons, and that these twenty-two persons should be all *Germans*[1]!

Equally unworthy of belief, because indeed it is impossible to be true, is the assertion that the supposed contagion was contained in, and propagated from a piece of silk, said to be found in the possession of the first Frenchman, who died. We shall admit the existence of the Frenchman; and no one will deny the probability of a Frenchman dying with a piece of silk in his possession. But what evidence have we that this piece of silk was not the produce of Lyons, or Pekin, rather than of Smyrna or Constantinople? Let us, however, suppose, that the first person, who died of the plague, in St. Giles's, was actually a

[1] Fracastorius.

Frenchman; that a piece of silk was really found in his possession; that this piece of silk was not of the manufacture of Lyons, or Pekin, but of Smyrna, or Constantinople; that it had travelled from the Levant, via Holland, to Long Acre, loaded with contagion; and that this cargo of contagion, disdaining to infect any of the crews of the ships, who had been employed in stowing and unstowing it, at the intermediate ports, or any Dutchman, or Englishman, through whose hands it might have passed, at Helvoetsluys or at Wapping, had no sooner arrived at St. Giles's, than it fastened, with instinctive animosity, as it were, upon the first starved Frenchman, it met with, in the purlieus of Drury-lane, choosing him as the medium of propagating itself throughout the metropolis of the British empire. Could we admit these, and some other, impossible suppositions, it would still remain to be proved, that an epidemic, or pestilential disease is ever propagated by contagion.

Dr. Hodges gives a different, although a no less absurd account of this matter: "As to the origin of our pestilence," says he, "I do not hesitate to assert, upon the most irreproachable authority, it made its way into our Island in consequence of contagion; and that it was imported from Holland in goods brought from that country, where it had made great ravages a year before.

If any one should wish to penetrate farther into this source, I will *demonstrate* to him, if *faith* may be put in the *reports*, which have elsewhere circulated, that the germ of that plague was brought from Holland, in a Turkish vessel, in cotton, a merchandise, which faithfully preserves contagion." It is unnecessary to remark upon this extraordinary mode of "*demonstration*," by putting "*faith in reports*." In all this, there are no facts, no proofs, no reasoning. How, indeed, could a man be expected to reason coolly, who had taken contagion completely for granted, and was so terrified for infection, as to prescribe from his parlour window, for patients in the streets?

Would it not have been more rational to suppose that the same state of the atmosphere, which prevailed in Holland, the year before, existed in England, in 1665, and was, in both, the cause of the prevailing malady?

The disputes respecting the origin of the yellow fever of Philadelphia, of 1793, are striking instances of the errors, into which men inevitably fall, in reasoning from false data. Having all, as usual, taken the *existence* of contagion for granted, the physicians of Philadelphia only differed with respect to its *origin*. The College were of opinion that it was imported from abroad; Dr. Rush, that it was generated in

the city. I shall describe this dispute in his own words: "Public report had derived it (the contagion) from several different Islands; had chased it from ship to ship, and from shore to shore; and, finally, conveyed it, at different times, into the city, alternately by dead and living bodies; and from these tales, all of which, when investigated, were proved to be without foundation, the College of Physicians composed their Letter[1]. It would seem, from this conduct of the College, as if medical superstition had only changed its name, and that, in accounting for the origin of pestilential fevers, celestial, planetary, and demoniacal influence, had only yielded to the term '*importation*[2].'"

But it does not appear that Dr. Rush was more successful, in tracing the origin of this epidemic, than the College of Physicians. Like them he attributed the disease to contagion; but imputed the infection, instead of tracing it to a foreign origin, to the effluvia arising from a heap of damaged coffee, lying on a wharf. That the effluvia arising from damaged coffee, or any other putrid vegetable matter, should produce disease in its immediate neighbourhood, is suffi-

[1] Containing their opinion respecting the origin and treatment of the yellow fever. *Rush, on the yellow fever, p.* 21.

[2] Rush, on the yellow fever, p. 164.

ciently intelligible: but, that it should produce a disease, capable, by contact of the diseased person, with a person in health, of reproducing itself, is so palpable an absurdity, that it appears difficult to conceive how a man of sense, like Dr. *Rush*, could, for a moment, have yielded to the delusion. Were it at all necessary to enter into a refutation of the opinion that a heap of damaged coffee, laying on a wharf, could produce a devastating disease, throughout an extensive city, it would be found in the progress of the epidemic, which, it appears from Dr. Rush's own account, affected the inhabitants of distant streets as early and as generally, as those of the streets in the immediate neighbourhood of the coffee [1].

His arguments against the importation of the contagion, which he supposes to have produced the yellow fever of 1793, are equally strong against its existence: "The report of the College of Physicians," says he, "has served to confirm me in an opinion that the plagues, which desolated most of the countries in Europe, in former centuries, and which were always said to be of foreign extraction, were, in most instances, of domestic origin. Between the years 1006 and 1680, the plague was epidemic 52 times all over Europe. It prevailed 14 times in the 14th cen-

[1] Dr. Rush's renunciation of this error is stated in the preliminary discourse, at p. 72.

tury. The state of Europe, in this long period, is well known. Idleness, a deficiency of vegetable aliment, a camp life from the frequency of wars, famine, an uncultivated and marshy soil, small cabins, and the want of cleanliness in dress, diet, and furniture, all concurred to generate pestilential diseases. The plagues which prevailed in London, every year, from 1593 to 1611, and from 1636 to 1649, I suspect were generated in that city. The diminution of plagues in Europe, more especially in London, appears to have been produced by the great change in the diet, and the manners, of the people; also by the more commodious and airy forms of the houses of the poor, among whom the plague always makes its first appearance. It is true, these plagues were said by authors, to have been imported, either directly or indirectly, from the Levant; but the proofs of such importation were, in most instances, as vague and deficient, as they were of the West India origin of our late epidemic[1]."

In 1793, the yellow fever appeared also in different parts of the West Indies[2]. In the Islands, attempts were made to trace the contagion to the continent; on the continent, it was traced back to the Islands. In the same year, and in

[1] Rush, on the bilious remitting yellow fever, p. 165, 6.

[2] Chisholm, on the malignant pestilential fever.

the same season, the English colonists on the coast of Africa were seized with an epidemic, which proved fatal to a great number. It happened, just at this period, that a ship arrived from Bulam, on the coast of Africa, at Grenada, in the West Indies. It was concluded, at Grenada, that the contagion, no proof being required of its existence, was imported in this ship, from Bulam. At Bulam, if a ship had arrived there from the West Indies, the conclusion would, no doubt, have been reversed.

Thus are we perpetually running after the ignis fatuus of contagion, and attempting to reconcile contradictions; whereas, if we were to content ourselves with the simplest, and most obvious views of the subject, we should find the phænomena of epidemic diseases, which are upon that supposition so perplexing, of the most easy solution. What can be more obvious than, that, in the present instance, the same general causes, which produced unusual vicissitudes, or noxious qualities, of the atmosphere, in Africa, might have extended their influence to the West India Islands, and to America; and have produced, in all of them, a similar disease?

## CHAP. XII.

*The manner of commencement, spreading, and cessation of epidemic diseases, and their fluctuations, are incompatible with contagion, but explicable by changes in the properties of the atmosphere—course of the epidemic, which infested certain districts under the government of Madras, in* 1809, 10, *and* 11*—opinions respecting the plague at Scio—progress and fluctuations of the plague of London in* 1665*—of the plague of Marseilles in* 1720*—and of the recent epidemics of Cadiz, Malta, Gibraltar, and Malaga.*

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In every pestilence, some particular quarter of a town, or of a district, is first and principally affected; whilst the others are attacked, not in regular succession, according to their proximity to that quarter, or to the intercourse of their inhabitants. The inhabitants of other quarters, until the air becomes vitiated with themselves, will be affected, not in the degree of the intercourse of the inhabitants of the quarter first affected with *them,* but in the degree of *their* intercourse with the *air* of the quarter first affected. The visits of the sick to the quarters, in which the air is not yet vitiated, will be followed by no increase of disease amongst the inhabitants of these quarters; but the visits of the inhabitants of these

quarters, to those, in which the air is already vitiated, will be attended with a certain increase of sickness. It is of the greatest importance, toward preventing epidemic diseases, to attend to this distinction; and, when they occur, toward limiting their ravages.

In countries, subject to epidemic diseases, the progress of the vitiated atmosphere is often in an ascertained direction. The fever, which, in 1809, 1810, and 1811, afflicted the districts adjacent to the Pylney Mountains, in the East Indies, travelled with a certain degree of regularity, in one quarter, from North to South, and, in another quarter, from South to North [1].

At Scio, the inhabitants have no fear of the plague from Smyrna, although within a few hours sail; but, when they hear of its being at Alexandria, which is at a much more considerable distance, but in a different direction, they expect it to a certainty to attack them [2].

The great plague of London, from its first appearance, in November, 1664, to its final establishment, in May, 1665, underwent fluctuations altogether incompatible with the progress of a contagious malady. It ceased, recommenced, subsided, and reappeared, repeatedly, in the fol-

[1] Report of the Committee.

[2] Narrative of a Journey in Egypt, and the Country beyond the Cataracts. By Thomas Legh, Esq. M.P. p. 5.

lowing manner. Between the first two deaths and the third death, from plague, there intervened a period of about a month; and between the third and fourth, six weeks; so, that, in ten weeks, there died only three persons of that malady, although the burials from all diseases had risen in the time, to nearly double the usual number. In the week ending the 24th of January, 1665, they amounted to four hundred and seventy-four, the usual weekly number being then about two hundred and forty. This was the greatest number of deaths which had occurred any week since the plague of 1656, and there was not one plague case amongst them. The fourth death from plague did not happen until the 12th of February; and from that period, none happened until the week ending the 25th of April, an interval of upwards of nine weeks, or nearly twice as long as the utmost assigned duration of the supposed capability of communicating infection!

Here, then, we have three distinct intervals, one of four, another of six, and another of nine weeks, between the successive deaths from plague. It ceased and recommenced three several times, in little less than five months, before it was finally established. During that period, the plague had been wholly confined to St. Giles's. It was not until the week ending the 2d of May, five months

from its first breaking out in that quarter, that any death from plague took place in any other part of London. There died that week eight of the plague at St. Giles's, and one in the city: but none elsewhere. This death happened in Bearbinder-lane; and the subject was said to be a Frenchman, who had removed from St. Giles's. It does not appear to have even been alledged that the disease was communicated by his means to any other person. It was not, indeed, till six weeks afterwards, viz. the week ending the 13th of June, that any more deaths from plague took place in the city, when there died four. In this progress, it is curious to observe, that, although nine died of the plague in the week ending the 2d of May, no deaths took place from that disease the following week, and but three the week after[1]. This faculty of subsiding and resuscitating is wholly incompatible with the nature of a contagious malady. When we find, that, whilst only four died in the city, sixty-eight died in St. Giles's, and none in Southwark; and when we consider, that it was after the ravages of the disease had considerably subsided in the western

[1] Instead of referring to the pages of the work, in which these facts may be found, I have thought it better, it being out of print, to insert a table in the Appendix, in which the reader will see, at one view, the progress of this dreadful pestilence.

parishes, that it began to spread in an alarming manner in the city, the eastern parishes, and the Borough of Southwark, the conclusion is inevitable, that this progress depended upon the state of the air, or other circumstances peculiar to these different quarters. And it seems highly probable, that, the four persons, who died of the plague in the city, on the week ending the 13th of June, as well as the person who died first of that malady in Bearbinder-lane, were either stragglers from St. Giles's, or inhabitants of the city, who had been casually exposed to the air of that quarter.

Upon this occasion, the disease appears to have observed three distinct periods, attacking the capital as it were in three distinct points. In the western parishes, as St. Giles's in the Fields, St. Andrew's, Holborn, St. Clement Danes, St. Martin's in the Fields, and Westminster, it came to its height about the middle, and declined toward the end of July. In the north-western suburbs, as Cripplegate, St. Sepulchre's, St. James, Clerkenwell, St. Bride's, and Aldersgate, it came to its height towards the middle, and declined towards the end of August. In the eastern suburbs, the city, and Southwark side, it did not come to its height till toward the middle, nor decline till toward the end of September. It is to be remarked, that, whilst the disease was raging in the

West, toward the middle of July, and there died in the two parishes of St. Martin's and St. Giles's in the Fields, 421 of plague, there died of it, in the city, but 28; in Southwark (including Lambeth) but 19; in Aldgate but 4; in Whitechapel but 3; and, in Stepney, but 1; and that, even after it had begun to decline in the western parishes, and to approach toward its utmost violence in the north-western suburbs, in the beginning of August, the city, Southwark, Stepney, Whitechapel, Aldgate, Wapping, and Ratcliffe, were very little touched. In the week ending the 1st of August, there died, in the parishes of Cripplegate and Sepulchre's only, more, by 48, than in all the city, all the eastern suburbs, and all the parishes of Southwark, put together.

But this geographical progress would have been quite impossible, had the disease been contagious; for the city, with which the inhabitants of all the other parts of the metropolis kept up a constant intercourse, would, in that case, have been one of the very first places infected, and would necessarily have become the very focus of infection. From this continual intercourse of the inhabitants of all the other quarters of the town with the city, the infection of a contagious malady, in whatever quarter it might have originally broken out, would be instantaneously brought to the Exchange, and be from thence

circulated with as much rapidity as the news of the day. But, as the inhabitants of the city were under no necessity of going upon business to St. Giles's, and as they would not choose to visit any place unnecessarily, whilst labouring under the calamity, or even the suspicion, of a pestilence, the reason is obvious, upon the presumption of the air being the cause of the malady, why the city, Southwark, and the eastern parishes remained exempt so many months, after the disease had been committing its ravages in those of the West. It is also obvious that the case would have been very different, had the city been the part first affected. In that case, the inhabitants of every other quarter of the town, from their almost daily resort to that center of business, would be frequently exposed to the influence of the noxious atmosphere, under which a great many of them would of course suffer; so that the inhabitants of all parts of the town, who might be in frequent communication with the city, would be affected at the same time, rendering it still more difficult to distinguish the cause.

Fortunately the progress of the pestilence was different. The city was not only the last place that suffered, but it suffered less, in proportion to its population, than any other part of the town. The 228 deaths from plague in the city, and the 205 in Southwark, in the weekly bills of the 1st

of August, can only be regarded as having principally happened amongst stragglers who had fled from the western, or north-western parishes, to the city, or Southwark side, with the disease upon them; or such of the inhabitants of London and Southwark, as had had occasional intercourse, I do not mean with the inhabitants, but with the air of those parishes. The air of the city, and of the Southwark side, must, indeed, have begun, at this period, to have become vitiated; and some few might have owed their malady to that source. But it was not till toward the end of August, or the beginning of September, that it became vitiated to a considerable degree. Then, however, when the disease had greatly declined in the north-western suburbs, and had almost wholly subsided, in the western parishes, it began to rage with the utmost violence in the eastern suburbs, the city, and the Southwark side of the water. Of the 7000 deaths from plague, which happened in the week from the 12th to the 19th of September, 5000 were in this quarter. In the following week, from the 19th to the 26th of September, the numbers were, in the city, the eastern suburbs, and Southwark side, 4116; in the north-western suburbs, 811; in the western parishes, 606. The relative numbers, in the week ending the 3d of October, were, in the

city, &c. 3724; in the north-western suburbs, 604; in the western parishes, 601.

It is not only the fluctuation, which marked the plague of 1665, in its commencement, and the circumstances which distinguished it in its progress, but the extraordinary manner, in which it suddenly declined and ceased, and the phænomena relating to health, which characterised some of the years immediately succeeding, that serve distinctly to indicate its real source.

Its fluctuations, and its progress in the different quarters of the metropolis, have been already described. With respect to the manner of its termination, it appears from history, that it was after the shutting up of houses, and all other precautions had been abandoned in despair, and whilst from thirty to forty thousand persons were still labouring under disease, that its farther propagation, at the usual season of termination, suddenly declined and ceased [1].

We are also informed that "the general yearly bill (of mortality) mentions a scattered plague of a few in every year, till 1680, except 1670 [2]."

Whilst these circumstances must be admitted to be wholly irreconcileable with the progress of contagion; it is perfectly evident that the noxious

[1] History of the Plague, by H. F.

[2] City Remembrancer, Vol. I. p. 456.

qualities of the air may be so partial, or fluctuating, and may increase or diminish, so gradually, or so rapidly or instantaneously, as satisfactorily to account for the phænomena attending the commencement, progress, and termination of pestilential maladies.

The manner of spreading of a disease, which depends upon the state of the air, as connected with winds, soil, seasons, temperature, moisture, &c. must be various, under different circumstances: and, although, by unprejudiced understandings, the connection might perhaps, in a general way, commonly be traced with sufficient accuracy, yet, to the mind pre-occupied with ideas of contagion, the connection between epidemic diseases and their real causes must always appear intricate or obscure. But if we take the facts communicated to us on this subject, for a different purpose, by the advocates for contagion, and consider them abstractedly from their doctrines, we shall in general be enabled to form not inaccurate conclusions.

At Marseilles, accounts were received, in May 1720, that from the month of March, (the usual period of their commencement in those countries,) the plague was rife in most of the maritime towns of Palestine and Syria. In May and June, vessels arrived, as usual, at Marseilles, from different ports of the Levant, and some with

clean, and some with foul bills of health; but no suspicious sickness. On the 12th of June, a quarantine guardian dies; his body is examined; and report is made that he bears no marks of contagion. On the 23d and 24th four servants employed in purifying goods at the infirmaries, are taken sick; and on the 24th and 26th all four successively die. Their bodies are examined, and report is made that they have no marks of contagion. Alarm spreads, and additional precautions are taken. On the 7th of July two more servants, employed to purify goods at the infirmaries, fall sick: but the surgeon does not consider their disease to be the plague. On the 8th another servant falls sick. A consultation is held: the disease is declared to be the plague. They die; and are buried in lime. On the 9th, 10th, and 11th, reports of several suspected cases are made, all from the Place de Linche. But these are afterwards denied; and no new cases are reported until the 26th of July, when fifteen persons suddenly fall sick in the Rue de Lescalle, a part of the old town inhabited only by poor people. Physicians and surgeons are sent to examine them. Some report that the disease is a malignant; others a contagious pestilential fever; none positively that it is the plague. The persons seized are confined to their houses, and afterwards sent to the infirmaries. On the

27th, eight of the sick die, and buboes are found on two of them. The physicians continue to think it not the plague. On the 28th other persons in the same street fall sick; and some of those, who first sickened, die. All communication with the Rue de Lescalle is interdicted. On the 31st intelligence is received that the Chamber of Vacation of the Parliament of Aix had published a decree forbidding the people of Marseilles from going beyond the bounds of their territory, and the inhabitants of Provence from communicating with those of Marseilles! 1st to the 8th of August occupied with measures of police, digging pits to bury the dead, establishing pest houses, and concerting measures for supplying the city with corn. 9th. Some of the physicians, and almost all the surgeons had fled. Two physicians, strangers, of the name of Gayon, come to volunteer their services, and to be shut up in the hospitals, where unhappily death too soon puts an end to their charity and their zeal. 12th. Messrs. de Chicoyneau and Veruy, physicians of Montpellier, come by order of the Regent, to enquire into the nature of the malady. Hitherto the disease had fallen only on the poorer sort of people. It killed all those it seized; hardly one escaping; and whatever house it entered, it carried off the whole family. 16th being the festival of St. Roch for imploring deliverance from the plague, is solemnized in the

usual manner. 17th. The physicians of Montpellier, in plain terms, declare the disease to be certainly the plague. People desert the city in crowds, so that the gates are hardly sufficient to let them out. 18th. Obliged to employ twenty-six galley slaves to bury the dead. 20th. Some of them taken ill; obtain thirty three more. Millers and bakers cease to work. 21st. The pestilence rages, the dead cannot be buried, divine service ceases in the churches. 23d. The deaths amount to near a thousand a day. Twenty slaves more are furnished to bury the dead. 25th. The plague is spread into the four corners of the city. From this period to the end of September, it continues to rage with the same violence, carrying off above a thousand persons a day. The heroic exertions of the Bishop, the magistrates, and many individuals, are here described with proper praise. On the 10th of September, a reinforcement is received of galley slaves; and six, who are butchers by trade, are sent to serve in the slaughter-houses, where, all the butchers being either dead or deserted, no one is left to kill oxen and sheep. By the 11th, there are hardly any physicians or surgeons, who have not run away or perished. The people are in want of every thing. Provisions are not to be procured. No medicines or drugs are to be had, owing to the flight of the apothecaries, druggists, and grocers.

The dying cannot make their wills for want of notaries; they cannot confess for want of priests. Women with child have no assistance in their labour. 24th. Misery is at its height. Those, whom disease has missed, perish by famine, or despair. The fountains of charity are dried up. "*The heavens seem to be of brass, and the earth of iron.*" 26th. A most violent north wind. 7th October. The plague is more violent in the territory than the city. 11th. Some of the patients in the hospitals begin to recover. 20th. No bell having been rung in the town since the plague, the curfew is ordered, by the Governor, to be rung as formerly. 26th. Though the plague has decreased, want of provisions increases. The distemper having got into the neighbouring places, and even into the capital of the province, hardly any corn or other necessaries, are brought to the markets at the barriers. 27th, 28th, and 29th. The disease has considerably decreased. There is no farther occasion for the multitude of surgeons that came from all parts, in consequence of the advertisement of the 30th of September. 1st November, being the feast of All Saints, the Bishop holds a procession, and pronounces a solemn consecration of the city. St. Charles did the like formerly at Milan, on a similar occasion. The first half of this month is employed in regulating the supply of provisions. 16th. The plague still continuing, the Bishop

resolves to exorcise it. 19th. The distemper, which had extremely decreased, having again increased a little, it was imputed to communication in the churches, and they were ordered to be shut up. 24th—27th. The plague still continuing in the territory, ordinances are published for regulating their communication with the city. At the beginning of December, no new patients having been taken to the hospitals, measures are taken for purifying the houses; those, who had fled, begin to return; confidence to revive; and the shops to be re-opened. But, it being still accounted dangerous to open the churches, the Bishop orders altars to be erected in the streets, and mass to be said at them in public.

In giving this extract from a journal of the plague of Marseilles, I have not thought it necessary to refer, page by page, to the original. The facts, which it states, bear internal evidence of authenticity: and I shall now proceed to deduce my inferences from them. 1. It may be observed that the progress of the malady had no connection, even in appearance, with the sickness of the servants employed to purify the goods in the infirmaries. This cannot of itself be a particularly healthy operation; and, unless their situation be otherwise more salubrious than such situations generally are, it is naturally to be expected, that, in epidemics, persons so employed,

should be amongst the first to be affected. But, from the ideas, which have so long prevailed, respecting the cause of these maladies, the illnesses of such persons have always been regarded with more than ordinary suspicion. 2. It was on the 26th of July, the ordinary season of commencement of epidemic diseases in such latitudes, that the pestilence of Marseilles was first distinctly announced, by the seizure of fifteen persons at once, amongst the poor inhabitants of the Rue de Lescalle; where it continued exclusively to prevail, till the 12th of August; it not being till the 25th that the four quarters of the city are declared to be affected. 3. The festival of St. Roch, on the 16th of August, instituted for the express purpose of imploring deliverance from the plague, is expressive of the period at which it usually rages with the greatest severity, in most of the countries of Europe. 4. The observance of the festival of All Saints, (the 1st of November) by the Bishop of Marseilles, and on a similar occasion by St. Charles, at Milan, is indicative of the period at which epidemics have usually shewn a manifest abatement. 5. This disease seems to have observed different periods of attack, upon different quarters of the city and territory, as I have described of the plague of London; for it first and mainly attacked the Rue de Lescalle; afterwards other portions of

the city, although in what precise succession does not appear from the journal; and finally, the territory, or suburbs, as I suppose, where it continued to rage with violence, on the 7th of October, after it had begun to decrease in the city generally. 6. It began to decline, as all pestilential diseases necessarily must do, but could not possibly do, if they depended upon contagion, the disease being capable of affecting the same person more than once, at the precise period at which the greatest number of persons are affected. 7. Its decline appears to have commenced, if not at the end of September, about the beginning of October; for we find that patients in the hospitals are stated to have began to recover on the 11th; and its abatement to have proceeded so far by the 20th, that the ringing of the evening bell, which had been discontinued, was, on that day, ordered to be resumed. 8. It wholly ceased in the course of November, there being no account of any new patients being taken to any of the hospitals, in the month following. 9. Thus the commencement, progress, and termination, of the celebrated plague of Marseilles, happened according to all the known laws of epidemics: for this disease, as well as the fever of London in 1665, and those of Malta, Gibraltar, Cadiz, and Malaga, here treated of, are all of the nature of plague,

or pestilence, as much as the fevers of that name, which usually occur in the Levant: And the absurd stratagem of denying them to be plague, lest they should be supposed contagious, resulting from an ignorance of their nature, cannot be sufficiently deprecated.

10. In no instance are the pernicious consequences of the belief in contagion more conspicuously manifested, than in the disasters, which happened in Marseilles, upon this memorable occasion; whether we consider the confinement of the inhabitants to the air, which was the main cause of the malady; the scarcity of provisions; the want of medical, and all other attendance; or the dejection of mind, and the almost certain destruction, with which such a belief can rarely fail to be accompanied. But this shall be afterwards more fully elucidated under the head of "injurious consequences of the belief in contagion."

In Cadiz, in 1800, it was in the Barrio de Santa Maria, "amongst the New-Castilians, strangers to the place," says Arejula, that the fever first broke out[1].

"In this quarter of the town the streets are narrower, less ventilated, and not so clean as in all the other parts, and here *the poorer inhabi-*

[1] Sir James Fellowes, p. 41

*tants, dirty in their persons, and crowded in filthy rooms, generally live together* [1]."

The disease, we are informed, continued to make gradual progress in this district; and, by the middle of August, the number of deaths amounted to twenty-five, or thirty a day [2].

The people became so alarmed that they called for religious processions. One took place on the 5th of August. It passed through the streets of Boquette and Sopranis, the places principally affected. And an immense concourse of the most infirm and ailing of the inhabitants, who are always the first to join in such processions, were *exposed, for seven hours, to a scorching sun.*

In five days after, cases of fever were reported in the other quarters, and on the 28th of August there were 157 deaths in Cadiz [3].

In March, 1810, an extraordinary mortality took place, on board of the pontones or hulks, in Cadiz Bay, amongst the French prisoners of General Dupont's army, who had surrendered at the battle of Baylen. Towards the end of June, the sickness was officially announced to have terminated [4].

On the 11th of September, 1810, the usual

[1] Sir James Fellowes, p. 7, 8, 33, 34. [2] Id. p. 37.
[3] Id. p. 41. [4] Id. p. 209.

fever was distinctly recognised at Cadiz [1]. On the 31st of October, the mortality had increased to fifty-two a day. On this occasion also, if it did not commence, the disease principally prevailed, as may be inferred, in the Barrio of Santa Maria [2].

Early in September, 1813, the fever again began at Cadiz, in the Barrio of Santa Maria [3]. And we learn, that, in 1730, and 1731, when it raged with violence, accompanied with black vomit, and other indications of unusual severity, this devoted quarter was the principal theatre of its ravages: for, in stating the opinions of the practitioners of Cadiz, respecting its cause, particular mention is made of those "at the Puerto de Santa Maria, *who saw and treated the disorder* [4].

I shall here quote from Sir James Fellowes, some observations, respecting the constitutions of these years, which are generally illustrative of the cause of epidemics: "The year 1730 was remarkable for the drought that was supposed to have occasioned the influenza, which prevailed very generally at that time; some barometrical observations, shewing the increased weight of the atmosphere during the period, made by Gott-

[1] Sir James Fellowes's Reports, p. 216.
[2] Id. p. 220. [3] Id. p. 256. [4] Id. p. 23, 24.

libius Ephraim, in his treatise " de efficaciâ aëris in corpore humano," corresponded with those made by Fernandez Navarrette in Castile and Andalusia.

" This author wrote to the Spanish physicians, exhorting them to give a history of the catarrhal constitution (which returned in 1738), to investigate its origin and causes, and to point out such measures as might tend to prevent the disorder (la peste) which then threatened all Spain. This work (8vo) was dedicated to the Royal Academy of History at Madrid in 1738 [1]."

It is not to be imagined, according to the doctrine, which considers the air the main cause of epidemic maladies, as connected with the other circumstances that have been mentioned in Chapter I, that those diseases necessarily commence, principally prevail, or at all appear, although that is generally the case, in those quarters, which are usually the most unhealthy. If the fever of the season, at Cadiz, should occasionally fail to appear at the Barrio of Santa Maria, it would not necessarily follow, that epidemic diseases do not generally first and principally attack those places, which enjoy the least salubrious air, and are inhabited by the most miserable classes of the community.

[1] Sir James Fellowes's Reports, p. 25, note.

The progress of sickness and mortality, on these occasions, may depend upon such accidents as the course of the winds. They may travel from south to north, or from north to south, or in the direction of any point of the compass. But, other things being equal, they always first attack and principally prevail in those spots, which are the lowest, and most unhealthy, and the habitation of the greatest human wretchedness.

By these principles, we may readily explain the progress of epidemic maladies, even when it is very different in places immediately adjacent. Thus, in the plague of Malta, in 1813, the greatest mortality took place at La Valetta in July, whilst at Casal Zebberg, not five miles distant, it happened in August; and so of other casals, or villages, throughout the island. The disappearance of the malady, in all, was attributed by the believers in contagion, to the exertions of the police!

It may be remarked of those, who, on the other hand, doubted, or disbelieved, the existence of contagion, in this particular epidemic, that their inferences must have been founded on the most partial and imperfect view of the subject, since they have thought it necessary, in order, as they imagined, to render their opinion consistent, to contend that the epidemic of Malta

was not the plague. It was certainly as much the plague, if any confidence may be placed in identity of symptoms, as any malady which has ever been seen, or described, in any part of the Levant. These persons, having their minds pre-occupied with the general error that the plague depends upon contagion, but seeing reasons for consluding that the pestilence of Malta did not arise from that source, found it necessary to maintain, notwithstanding its decided symptoms, that that malady was not the plague :—a most egregious error, and a source of much pernicious delusion.

The fever of Gibraltar, in 1804, according to Sir James Fellowes, first shewed itself amongst the foreign inhabitants of the lower town, in a range of houses called Boyd's Buildings, and for some time appeared no where else [1]. It was three weeks before it shewed itself in the south [2]: and nearly a month before any of the 54th regiment, stationed within one hundred and ten yards of this building, were affected [3].

Some of the first persons reported to be seized, were artillery-men, and their wives, who had frequented Boyd's Buildings [4]. By the end

[1] Reports, p. 90, 105. [2] Id. p. 132.
[3] Id. p. 91. [4] Id. p. 102.

of September almost the whole of the town was affected [1].

It happened that one of the persons first seized in Boyd's Buildings, had come from Cadiz, where the fever also prevailed; and he was therefore considered, by the believers in contagion, *i. e.* by almost the whole community, as the medium by which the malady was introduced to Gibraltar. Respecting these circumstances a minute examination took place, the particulars of which are related by Sir James Fellowes, to whose Reports I refer [2].

The impression made upon my mind, by the whole of this narrative, is, that, at first, those persons only were affected who frequented certain unhealthy districts, as Boyd's Buildings, and Rosia's Guard; those who were in less insalubrious stations, but did not frequent these quarters, remaining exempt from disease, even when visited by the inhabitants of the quarters principally affected: and, that this continued to be the case, until the air throughout the garrison generally became vitiated, when it ceased to be practicable to trace the origin of the sickness

[1] Reports, p. 91.

[2] Id. p. 102, 103, 117—121, and Appendix B. p. 454, 455.

in particular cases to connection with particular quarters [1].

A circumstance of a singular nature, mentioned by Sir James Fellowes, upon this occasion, respecting mortality at Gibraltar, well deserves to be repeated. "The usual military establishment of the garrison amounted to 4000 men, and the average number of deaths for the six years preceding the one when His Royal Highness the Duke of Kent entered on his command, was about seventy-two, the year of sickness already referred to (1804) excepted.

"During the short period of a twelvemonth that the Duke continued in Gibraltar, there were not more than twenty-seven deaths, and in the last six months only twelve. This is recorded as a remarkable fact, and would prove either the salubrity of the climate, or the great attention, which was paid by His Royal Highness to the health of the troops [2]."

The circumstances are indeed extraordinary; for, whilst seventy-two deaths in 4000 give only one in $55\frac{1}{2}$, or nearly one half less than the ordinary mortality in civil life, twenty-seven deaths in 4000 gives only one in 148; or not quite one-fourth of the ordinary mor-

[1] Reports, Pref. p. xix. and p. 8, 120, 132—137.

[2] Id. p. 87.

tality in civil life; reflecting the highest credit upon the administration of His Royal Highness.

Having requested information upon this subject, from a gentleman, who resided on the spot, and is an attentive observer, he states, that the unusual healthiness of the garrison, during the short period of the Duke of Kent's command, may, in his opinion, be ascribed chiefly to "the suppression of many wine-houses, and to the establishment of regimental-canteens, by which the soldiers were prevented from straying from their barracks, or sleeping in the sun to sober themselves; *to the introduction*, in short, *of a system of sobriety, till then very little known upon the rock.*"

Eight thousand persons that were turned out of Gibraltar, at the commencement of the epidemic in 1813, and encamped upon the neutral ground, continued in health; whilst, of those, who remained within, 2847 were seized, between the 8th of September, and the 3d of December, with the prevailing fever; and of these 904 died [1]. This fact is of course liable to be interpreted differently according to the opinions which may be entertained of the cause of the disease. Were not our proofs, upon the general grounds, so com-

[1] Reports, p. 452, 3.

plete, as to render refutation in particular cases superfluous, it might be easily shewn, from the circumstances of this case, that the disease did not depend upon contagion.

Of the fever at Gibraltar in 1813, Mr. Gardiner, Surgeon of the Naval Hospital, observes: "that the disease did not spread from any focus, but broke out in fifty different places at once [1]."

"The rise and progress of our epidemics," (at Gibraltar) says Mr. Amiel, "have never been traced in a satisfactory manner, from a single point of contagion, to a gradual number of individuals, or families; and instead of creeping slowly from one district to another, cases have made their appearance unconnected and scattered at different points; and in some instances, it has spread with the rapidity of the electric fluid, attacking persons who had never approached the sick, or any assignable source of contagion [2]."

In the Appendix, a table is inserted from Dr. Burnet [3], by which the reader will be enabled to trace the fluctuations which marked the fever of Gibraltar in 1813. It commenced on the 8th of September. From that to the 19th, the daily number taken ill fluctuated from four to nine. On the 20th, the number suddenly increased from

[1] Burnet, p. 312. [2] Id. p. 324. [3] P. 341.

six to thirty-three, the weather being very hazy and close. On the four following days, the numbers stood, nineteen, thirteen, eighteen, fourteen. On the 25th, the number increased four-fold, or from fourteen to fifty-seven, and on the 26th rose to seventy. On the 27th and 28th, it again decreased from seventy to fifty, and from fifty to twenty-nine. From the 29th to the 4th of October, the daily increase of the sick varied from fifty-four to thirty-five; and from that period to the 12th from forty-eight to eighty-six. On the 13th it suddenly rose from sixty-three to one hundred and fifteen, from which day it again gradually decreased, until the 21st, when it fell at once from seventy-one to twenty; the wind having changed from East to South-West, with heavy rain night and morning. After that day, it never rose beyond forty-six, but kept varying from that to ten, till the 9th of November, when it fell to nine, from which it gradually declined, until it wholly ceased in the beginning of December. These are fluctuations, which are wholly incompatible with the progress of a contagious malady, but are easily to be accounted for by changes in the properties of the atmosphere. In 1813, as the greatest sickness (115) took place on the 13th, so the greatest mortality (37) happened on the 17th of October, four days afterwards. In 1804, the

greatest mortality (170) was on the 9th of October [1].

This was the progress of the fever of Malaga in 1803. A smuggler, of the name of Felix Munōz, was reported to have died of it about the 20th of July [2]. Between this, and the next case, there was an interval of thirty-five, or thirty-six days. It happened in the family of Christopher Verduras, a caulker by trade, but a noted smuggler, residing in the district of Perchel, on the west side of the Guadal-Medina. Michael Verduras, the son, who was the first attacked, was seized on the 26th of August, and died on the 3d of September. Several members of the same family were afterwards taken ill, and the disease continued principally to affect the inhabitants of the narrow lanes and streets of the district of Perchel. "But it afterwards extended to others of the suburbs, and to the adjoining district of Trinidad, Capuchins, and Alto, which is at some distance from the Perchel, owing, it was said, to these last, principally labourers and workmen, who joined early in the morning to hear mass in the church, with those of the Perchel district, which was the first open, when that ceremony was performed daily [3]."

"It was affirmed by the inhabitants of the dis-

[1] Burnet, p. 205. [2] Reports, p. 163, 4. [3] Id. p. 167.

trict of Perchel," says Arejula, "that those who had entered the church of St. Peter to hear mass on St. Michael's Day, were all taken ill, and a great part of the congregation died [1]." The custom, amongst Roman Catholics, of going early in the morning, before going to work, into a cold damp church, to hear mass, and of remaining there for some time, is one of the most appropriate modes of exposure to the causes, which produce an epidemic. In Catholic countries, and especially in Spain, the labouring people are very strict in their observance of religious ceremonies. And, this, as I shall have occasion to shew, is, upon a principle very different from its propagating contagion, a fertile source of pestilence.

It may be proper to give, from the same author, a short description of the city of Malaga. It is situated in latitude 36°. 44′. "on the coast of the Mediterranean, in the centre of a bay, formed by a chain of mountains that run from East to West (except that part towards the North West, which is a plain of low pasture of four leagues in extent) and is surrounded by high hills, some of which are so near, that a portion of the town is built upon them.

"From the Northern part, the river Guadalmedina issues from between the mountains, and

[1] Reports, p. 168.

passes with a rapid course through the middle of the city, leaving the district or Barrio de Perchel and Trinidad to its right, and the remainder of the town to its left.

" In consequence of an inequality in the depth of the bed of the river, occasioned by frequent torrents in the mountains, a part of those districts, and the whole of the lower town, are frequently inundated. Even with the least torrent of the Guadal-medina, the streets are overflown, which, upon the waters retiring, are left full of mud and clay.

" Dr. Mendoza, in his 'History of the Epidemics of Malaga, in 1803, 4,' says, that, since that period, measures have been taken to prevent the water entering the lower part of the town [1]."

After this description, it ought not to surprise us to find that Malaga has been often visited by the plague. The circumstances here indicated, together with the uniform commencement and disappearance of the disease, at particular periods of the year, concur in shewing both what is, and what is not its cause. Its progress through the different branches of the different families, when it first attacked in the Barrio de Perchel, is described with commendable minuteness and accuracy by Sir James Fellowes [2]; and it is but jus-

[1] Reports, p. 157, 8.

[2] Id. p. 169, et seq.

tice to that gentleman to add, that the integrity of his facts do not appear to have been in any sensible degree influenced by the nature of his opinions. But, with whatever confidence I rely upon his facts, I must take the liberty of directly controverting his conclusions. Notwithstanding all that has been mentioned upon this occasion, it is to me quite evident, that the attacks of the persons mentioned happened, not because they might have been noted smugglers, and visited strange ships, but because they resided in the Barrio de Perchel, perhaps worked hard, certainly lived low, and because the season was the autumn.

If it be true that in 1804, the disease appeared in an opposite quarter, the Pozos Dulces [1], and did not break out till two months afterwards, viz. in the beginning of September, in the Barrio de Perchel, the reasoning, which I have already employed in treating of the epidemics of Gibraltar, will equally apply to this; and it is only as if the plague should commence, in London, one year in St. Giles's, and another at Whitechapel.

[1] Reports, p. 177.

## CHAP. XIII.

*In epidemic diseases, the removal of sick persons, from the noxious, into pure air, is not followed by any propagation of the disease--remarkable instance of this at Larnica, in Cyprus, related by Dr. Russel.—Mr. Pym's curious doctrine.—Persons ill of the plague came from the mountains to the sea-port towns of Antioch, Shogre, and Edlib, and the disease was not propagated in these places.—Similar circumstances related by Dr. Rush, in respect to yellow fever, at Philadelphia; by Dr. Pye, of a pestilence of Santa Cruz, in Teneriffe.—Also of the epidemics of Gibraltar, by Dr. Burnet, Mr. Lamert, Mr. Amiel, Mr. Glasse, Mr. Martindale, and Mr. Playfair.*

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THERE is something wonderfully methodical in this matter of contagion, in never seriously beginning its march, or taking the field in force, but at certain periods of the year, doubtless those which are most favourable to its unwarrantable designs, and in which it may hope to catch the police institutions napping; as well as at periods equally determinate, in finishing, its campaigns, and retiring into winter quarters. It is also, it seems, very particular, or, as Mr. Pym chooses to express it, "*fanciful,*" in respect to the kind of atmosphere, in which it deems it expedient to make its attacks;

to that of Larnica, for instance, it, one year took such an unaccountable dislike, that it would not touch a single person in the town, although, on the following year, it slew them by twenties and thirties of a day, routing those, who would not stay to be slain, and driving them to the mountains. But Mr. Pym shall relate those vagaries of the *contagion of plague* in his own words:

"But the plague is perhaps, more *fanciful* in its *choice of a particular atmosphere,* than any other disease; it is said to have a *dislike* for extremes of either heat or cold, and Dr. Russel mentions several instances of its being carried to places where *(for a time at least)* it lost its power of propagation:—at page 31, he observes, "That in the month of April, 1759, a Turkish vessel, from Alexandria, was wrecked on the island of Cyprus, and that a great part of the crew who were saved happened to be infected with the plague, that the contagion was spread with great rapidity to the towns and villages; but the town of Larnica, he says, at this period was remarkable. It had received a part of the infected crew, and had maintained a constant intercourse with the infected quarters of the island—peasants and mule-drivers, from those parts, with the pestilential sores on their bodies, were daily in the streets and markets, and some of them died in the houses of Larnica, two vessels also arrived, both of which

landed infected passengers and sailors. Notwithstanding this new importation, none of the inhabitants of Larnica were known to have contracted the plague, although it suffered severely from it the following year, in the months of February and March, when few or none of the infected recovered. The daily funerals were from twenty-five to thirty, and many of the inhabitants fled to the mountains."

He also observes, page 26, "So far is certain, that although infected persons came from the mountains to the three towns just mentioned, (Antioch, Shogre, and Edlib), and some of them died in the families where they lodged, yet the distemper, by such means, was not propagated: as if divested of that contagious property *in the plains*, which it seemed to retain undiminished *in the mountains*." And at page 61, "That the villages appeared to suffer in a singular degree, owing perhaps, to the structure of the huts and cottages, which are small, with few or no windows, and stand crowded together [1]."

Reasoning seriously upon this fact, it is one of very extraordinary value, since it affords most lucid demonstration of the non-contagious nature of plague, which is so seldom to be met with, and

[1] Pym's Obs. p. 202.

could have no where occurred excepting amongst Turks, or others who have no apprehensions of the malady. These men in a state of disease, enter a town, and have communication with its inhabitants, without the disease being produced in any of the latter. In other towns, in which parts of the same crew had disembarked, the disease spread rapidly. In what manner is this to be accounted for? Is Larnica insensible to the disease? It suffered from it the following year. Had the contagious influence ceased in that part of the crew, which visited Larnica? It had maintained a constant intercourse with the so called *infected* quarters of the island—peasants and mule-drivers, from those parts, with pestilential sores upon their bodies, were daily in the streets and markets, and some of them died in the houses of Larnica. Two vessels arrived, both of which landed diseased passengers and sailors. Yet none of the inhabitants of Larnica were known to have contracted the plague. Now, as there is but one rational mode of explaining these facts, why should we hesitate to adopt it, although it should militate against the existence of contagion? Or must we prefer that mode which is *not* rational, because otherwise we must abandon contagion? It is evident that in those towns and villages, in which the plague appeared, there was that year a

pestilential atmosphere, which would have produced the disease, whether the crew of the ship from Alexandria had landed or not; whilst, at Larnica, the atmosphere, not being pestilential, did not produce the disease, notwithstanding the importation from Alexandria, and the intercourse with the neighbouring, so called *infected* places; whereas the atmosphere of the same place, being, the following year, pestilential, did produce the disease, and would have equally produced it, with, or without intercourse with neighbouring places supposed to be infected. This, to my plain apprehension, is a very satisfactory explation, amounting indeed alone to a proof.

But the facts, respecting Antioch, Shogre, and Edlib, settle another point of this controversy. The advocates for contagion, who used to trace its progress with the same facility as they would trace the course of a ship, on a chart, were in the constant habit of landing it in bales of goods at the Custom House, and from thence scattering it by a regular routine over villages, towns, cities and provinces. The importation, they alledged, always took place from some distant part of the world, first into the sea-port towns, from whence the contagion was afterwards duly transmitted to the inland countries. But here, we have the plague arising in the mountains, and persons affected with it going to sea-port towns, and dying

there, without the disease being communicated to any one. Here is a double proof, that plague does not necessarily make its first appearance in sea-port towns, although it generally does; and that, as it is incapable of being propagated from the sea-port towns to the mountains, it cannot be capable of being propagated from the mountains to the sea-port towns. When a coincidence happens, it must depend upon the concurrence of similar states of the atmosphere in both.

The same observations are equally applicable to other epidemics. "It has been remarked," says Dr. Rush, in speaking of the yellow fever of Philadelphia, in 1793, "that this fever did not spread in the country, when carried there by persons, who were infected, and afterwards died with it." In another place, he observes, "during four times that it occurred in Charlestown, in no one instance, according to Dr. Lining, was it propagated in any other part of the state [1]." Convincing proofs that the disease did not depend upon contagion; but upon the states of the atmosphere, at Philadelphia, in the one case, and at Charlestown, in the other.

Dr. Pye says, that, "Mr. Fox, a merchant of London, lived in the Isle of Teneriffe, when a

[1] An Account of the bilious remitting yellow fever of Philadelphia, by B. Rush, M.D. p. 157.

plague raged with great fury at Santa Cruz, in that Island. The place he resided at was the city De la Laguna, about three or four miles from Santa Cruz: and he assures me, that though a great many diseased persons were brought thither, some of which too died there, and though the people of La Laguna visited and conversed with those sick persons, yet none were hurt thereby; nor was there a single instance of any person's having the disease, who had not been at Santa Cruz, the place where the sickness raged [1]." In 1810, a fever appeared at the usual pestilential season at Santa Cruz, in Teneriffe, which Sir James Fellowes alledges to have been imported from Cadiz: and, on this occasion, he observes, that "it was a very remarkable circumstance, that the pestilential fever should have broken out in one of the Canary Islands, *where such a disorder had never appeared before* [2]." The pestilence mentioned by Dr. Pye, must have escaped the observation of Sir James Fellowes. The fact, however, is of some importance in this discussion. It is to be regretted, that the season of the year has not been mentioned, at which the first epidemic of Teneriffe happened; that of 1810 took place in *October* [3].

[1] A Discourse of the Plague, p. 71.
[2] Reports, p. 227. [3] Id. p. 229.

The following facts, relating to the epidemics of Gibraltar, further confirm this conclusion. "The ships in the Bay also suffered more, or less, which, as they had no communication with the shore, but through the medium of the Health Office, places the morbid effects of the atmosphere, in a strong point of view; while those in Algesiras Bay entirely escaped [1].

The same thing happened, with respect to the crews of the British men of war, lying before Cadiz, in the fever of 1764, described by Dr. Lind. In both instances, the disease was milder than ashore.

The Partridge, of 20 guns, was sent into Gibraltar, with dispatches, at the time of the epidemic, being quite healthy. On her return to Lord Exmouth, off Toulon, she had a number of men attacked with fever; which the surgeon, in his Medical Journal, states to be of the same nature as that which prevailed on shore at Gibraltar, adding, "no communication whatever was held with the place, except by receiving a lightning conductor from the Dock yard, where they were known to be perfectly healthy; and some boxes of money were received on board from a gun brig, which had brought them from Cadiz. The brig had no sick whatever on board [2]."

[1] Burnet, p, 313. [2] Ibid. p. 313.

Mr. Lamert says, that, of the wardmasters, orderlies, and nurses, and other women, most of them selected from among persons who had already had the disease, but eleven of them who had never been attacked, although perpetually exposed to contact with the sick, yet *they all escaped,* as did his assistants, Mr. Brady 26th Regiment, Dr. Burke 37th, and Hospital mate Leonard [1].

Mr. Glasse, who served in Gibraltar for ten years, and witnessed the fatal fever of 1804, as well as those of 1810, and 1813, thinks "the same atmospheric influence, which produced it in other parts of Spain, caused it at Gibraltar, and it was there most fatal from the crowded and filthy state of the garrison [2]."

He also notices the "sudden and beneficial effects produced by the heavy rains at the epidemic season [3].

"An individual," says Mr. Amiel, "labouring under our epidemical fever, on being removed to a pure and ventilated place, such as the neutral ground, or Europa point, did not communicate the disease to those in the closest contact with him; this observation has been confirmed in many instances, during the epidemic of last

[1] Burnet, p. 323. [2] Ibid. [3] Id. p. 324.

year, amongst the foreign recruits quartered at the brewery barracks, &c.[1]."

Mr. Martindale, in the official replies, states: "Dillon's regiment was quartered in the blue barracks, near the Moorish Castle; a great number of the men took the fever, and several died; in consequence of which the regiment was sent out to the neutral ground, and encamped, and immediately the fever stopped[2]."

"The 8th battalion of the 60th regiment, arrived from Cadiz in a healthy state, and were encamped on the Governor's meadow. Shortly afterwards they went into town, and were quartered in the cooperage range; the fever instantly raged amongst them, and both officers and men suffered severely. They were then sent back to the encampment, and the disease, as in Dillon's, immediately ceased—as if by magic."

Mr. Playfair, Surgeon of Dillon's, has the following observations in his official replies: "It is very striking, that, even after removal to the camp in the Governor's meadow, while the men of Dillon's regiment were allowed to enter the town, on fatigue duty, the fever still continued to prevail; but from the day of their confinement to the neutral ground, they were quite free

[1] Burnet, p. 324. [2] Ibid p. 437.

from it, *although they had equal intercourse and communication with the inhabitants coming from town, in which the disease was at that time very destructive* [1]."

Dr. Burnet mentions three well authenticated instances, in which cases of the Gibraltar fever, in its greatest severity, were received on board of men of war, and the patients died, without any contagion following [2].

Of the fever of Gibraltar in 1810, which he attributes to marsh miasma, and which was believed by others to have been the Bulam fever resuscitated, he says: "A refreshing north west wind came, and scattering the noxious vapours over the face of the waters, restored health to the garrison [3]." "This also was observed in the fever of 1813. On the wind coming to the westward, with rain, an immediate decrease in the numbers taken ill was evident [4]."

"In the Leyden, lying in the bay (of Carthagena) nearly fifty of the 67th regiment were attacked, and, except one, they all recovered on board: only such men, as, by going on shore, were exposed to the marsh air, were taken ill, and they did not communicate the disease to a single person on board."

"With such powerful evidence of the non-

[1] Burnet, p. 437. [2] P. 267. [3] Id. p. 268. [4] Id. p. 269.

contagious nature of *this* disease before me," says Dr. Burnet, "I cannot but conclude, that it is neither propagated by persons, or goods, but is *the genuine product of miasma.* It is useless for any one to say, in support of its contagious qualities, that whole families have been attacked; this only proves that whole families have been *exposed to the same remote cause.* In Zealand, nearly whole regiments were taken ill with the fever, yet no one was hardy enough to assert that it was contagious [1]."

Dr. Burnet is here in error in more respects than one. There was at least one person, and at the head of the medical department too, hardy enough to assert, in an official return from the Island, that the fever amongst our troops at Walcheren was contagious! The emphasis on *this* disease, with other observations to be met with in the "Practical account of the Mediterranean fever," clearly indicate Dr. Burnet's belief in contagion in epidemic diseases generally, and that he only considers the fever of Gibraltar an exception. The doctrine of marsh miasma, as adopted by him and Dr. Bancroft, after Lancisi, Cullen, and others, is considered, and refuted, in treating of the cause of those maladies.

Mr. Amiel says, that individuals, labouring

[1] Burnet, p. 276.

under this disease, on being removed to a pure air, such as the neutral ground, or Europe point, did not communicate the disease to those in the closest contact with them [1].

Mr. Humphreys observes, that, the only measure (of prevention) found decidedly efficacious, has been the removal of the people from the western side of the rock [2].

[1] Burnet, p. 355. [2] Id. p. 357.

# BOOK IV.

## STATE OF OPINIONS IN THE 18TH CENTURY, AND RECAPITULATION.

## CHAP. XIV.

"Men espouse the well-endowed opinions in fashion, and then seek arguments either to make good their beauty, or varnish their deformity."

LOCKE.

*Separation, seclusion, and restriction, adopted in England, after the example of Venice, and other Catholic States—history of epidemic diseases since* 1547, *that of the devastations of the doctrine of contagion—history of the plague of London in* 1665, *best written by a sadler of Whitechapel—excellent journal of that of Marseilles, in* 1720, *by Mr. Pichatty de Croissante—works of the physicians less instructive, and why—quarantine first imposed upon British ships—merits, praises, and mistakes of Dr. Mead—his discourse concerning pestilential contagion composed of the jejune traditions of the Levant and of Italy.*

DURING the dark ages, the opinions, which prevailed respecting epidemic diseases, excepting where those of the ancients were known, consisted only of emanations from general or local

superstitions. Even after the revival of learning, down to the æra of the Council of Trent, being a period of about a century, although these diseases certainly did not cease to abound, no records of authority exist respecting them. In the year 1547, the æra of contagion commenced. I have not at present an opportunity of tracing the progress of this doctrine, throughout the continent of Europe, and especially in Italy, which may be considered as its mother country. But this defect, although the information might be both interesting and amusing, is not of sufficient importance, with a view to useful knowledge, to induce me to defer the publication of this work. That it spread rapidly, and that it met with no opposition, in Catholic countries, may, considering its source, be with certainty inferred: and we find, that, even in England, notwithstanding the indisposition of Henry VIII. toward the Pope, this doctrine, from the connection then still subsisting between medicine and the church, spread with little less rapidity, and was almost as firmly established as elsewhere. It gave rise here after the example of Venice, and other Catholic States, to those pernicious measures of separation, seclusion, and restriction, which, for a century, to the termination of the last great plague in London, continued to produce so much misery

and destruction, whenever an epidemic occurred. But it was not till after the plague of Marseilles, early in the last century, that the system of delusion was so far advanced, as to render practicable the imposition of a quarantine upon ships coming from the Levant[1]; nor until within a very few years, that it has been perfected, by the transfer of that ordeal, and of Lazarettos, &c. to our own shores.

To trace the history of epidemic diseases, from the æra of the Council of Trent, to the present moment, would be only to trace the progress of contagion, in all the melancholy variety of its devastating course. By stopping at some of the intervening stages, however, we are enabled to procure materials for the refutation, and to take a more accurate survey of the destructive consequences of this most pernicious of errors. The most edifying of these stages are perhaps the periods of the plague of London, in 1665, and and of that of Marseilles, in 1720; especially because authentic records exist, respecting their circumstances and progress.

It cannot fail to strike the reader as particularly curious, that, of all the histories which have

[1] This measure was at the time remonstrated against by the Levant Company.

appeared, of these great pestilences, those, which have been written by physicians, on account perhaps of the hypotheses to which they have deemed it necessary to adhere, have been by far the least instructive, or rather the most misleading; whilst writers, not of the medical faculty, have afforded the most solid and useful information. The former, having almost invariably taken a wrong departure from the rock of contagion, were ever afterwards unable to regain the right tract themselves, or to point it out to others. In the progress of their research, they experienced nothing but confusion, embarrassment, and defeat. Every object in their way assumed the colour of the fatal rock: whilst writers of another description, although from terror and habit equally believers in contagion, having no self-love embarked in the hypothesis, represented circumstances as they found them, without attempting to reconcile contradictions, and have therefore left upon record some unperverted facts,—some leading and useful landmarks.

Thus, in respect to the plague of London, whilst the works of Dr. Hodges, and the other learned physicians, who have treated of that epidemic, are calculated entirely to mislead; we find, in the journal of an obscure sadler of Whitechapel, to which are affixed the initials of

H. F. a surprising sum of applicative information, conveyed in the plainest way [1].

The same observations apply to the plague of Marseilles. Whilst the works of the physicians, who have written upon the subject, are, for the most part, loaded with frivolous disputes, hypotheses, or conjectures; we find, in the journal of an enlightened magistrate of that ill-fated city, complete data for the fullest and most satisfactory inferences, concerning its cause [2].

It is at the period of the plague of Marseilles, that, in glancing at the progress, and tracing the consequences of the doctrine of contagion, we ought to make our principal halt; both because the transactions of that period have been the

[1] This work, entitled "The History of the Plague," &c. to which I am indebted for the grounds of many inferences, respecting the epidemic of 1665, states, at p. 10, the author to have been a sadler, residing at Whitechapel, and an eye-witness of the calamities, which he has described. It bears unequivocal marks of being genuine. Upon what grounds it could have been attributed, as has generally been the case, to De Foe, who, if born in 1665, must have been in the first stage of infancy, I am wholly at a loss to conjecture.

[2] Journal of the Plague of Marseilles; which, although published anonymously, bears internal evidence of having been written by Mr. Pichatty de Croissante, the king's procurator, who had himself a considerable share in the councils of the city, at that calamitous period.

principal cause of prolonging the delusion generally for nearly another century; and because they have been the means of introducing into this country first the notions, and afterwards the reality, of those pernicious institutions of precautionary police, which had previously been established upon the continent.

The alarm excited in England, by the ravages of the plague of Marseilles, having come to its height, the privy-council held deliberations upon the subject, and Dr. Mead was requested by Mr. Scraggs, secretary of state, to draw up directions for its prevention. This was accordingly done. But, if we may judge by the work produced upon that occasion, either panic had swept away all powers of reasoning, or the doctor's powers were not correspondent with his reputation. His "Discourse," since so blindly admired, and so lavishly praised, we now find to have been composed of nothing but the fables of Fracastorius, Benedictus, and Forestus, as sanctioned by the Pope, and the Council of Trent:

> For fashion's sake, he seems to have recourse
> To Pope, and councils, and tradition's force.
>
> RELIGIO LAICI.

His ideas, however, are arranged with address, and related with elegance and precision [1].

[1] "A Short Discourse concerning Pestilential Contagion,

But, what can be expected from books composed, or directions issued, even by the most able writers, upon a subject, with which they are scarcely, if at all, practically acquainted (Dr. Mead had probably never seen a pestilential malady), when under the influence of the terror, inspired by the actual progress, or expected approach, of a devastating disease, a disease too supposed to be propagated by contagion, but the natural fruits of such a state of mind, error and delusion!

This sophistical, but orthodox, discourse of Dr. Mead, engaged the privy-council, or at least seemed to justify them, in the subsequent adoption of those measures of plague police, or more properly those pestilential measures of police, which originated nearly two centuries before, with the Council of Trent, in Italy, but had not until then been even thought of, in this country.

An answer to Dr. Mead's work, and what, by unbiassed judges, might have been considered a refutation of it, was, shortly after its appearance, published by Dr. George Pye [1]. But that, and

and the Methods to be used to prevent it. By Richard Mead, M.D. Fellow of the College of Physicians, and of the Royal Society, London, 1720.

[1] "A Discourse of the Plague, wherein Dr. Mead's

a hundred equally cogent refutations, would not, then, have been capable of producing the smallest sensation upon the public mind. The inveteracy of the established belief in contagion, the authority of Dr. Mead, and the influence of all-powerful terror, were quite sufficient, without the aid of the infallibility of the Pope, to extinguish a few feeble rays of emerging truth.

In thus commenting upon what I deem to be his pernicious doctrines, or rather the pernicious doctrines of others, strengthened by his authority, it will not be suspected, by any liberal mind, that I mean an invidious reflection upon the memory of the illustrious physician, who was thought worthy of being celebrated by the muse of Pope, to whom Dr. Friend addressed his learned "History of Physic;" whose praises are sung, in such strains as these, by the poet of health:

> Nor should I wander doubtful of my way,
> Had I the lights of that sagacious mind,
> Which taught to check the pestilential fire,
> And quell the deadly Python of the Nile.
> O! thou, beloved by all the graceful arts
> Thou, long the favourite of the healing powers,
> Indulge, O Mead! a well-designed Essay,

notions are considered, and refuted, by George Pye, M.D. London, 1721.

> Howe'er imperfect; and permit that I
> My little knowledge with my country share,
> Till you the rich Asclepian stores unlock,
> And with new graces dignify the theme.
> ART OF PRESERVING HEALTH, B. 1. l. 53.

And whose skill is gratefully and pathetically extolled by another poet:

> ..... And am I fond of life,
> Who scarce can think it possible I live?
> Alive by miracle! or what is next,
> Alive by Mead!
> YOUNG'S NIGHT THOUGHTS, N. 4.

Dr. Mead, unfortunately perhaps for medical science, was of such high reputation, as to have been regarded the general patron of learning, the very Mæcenas of his age. Amongst the numerous dedications, which I find have been addressed to him, there is one prefixed to Ainsworth's Latin Dictionary, in which he is thus eulogised: "Hinc, ut nobilis ille Asclepiades, M. T. Ciceronis medicus et amicus, utroque uteris Apolline, et tam orando vales, quam medendo." He was at any rate the indisputed Mæcenas of contagion: for, besides others, we find Dr. Joseph Brown, in 1720, dedicating to him his "Treatise on the Plague," and Sir John Pringle, about the middle of the century,

his " Observations on the Nature and Cure of Hospital and Jail Fevers."

It must be acknowledged that a more fit person, in all respects, to give grace and plausibility, to an absurd doctrine, could not have been readily found. Nor is it to be doubted, that, Dr. Mead must have felt, that he had rather an ungracious task to perform; since he was under the necessity of either adopting the traditions, which came to his hands, sanctioned by time, and confirmed by habit, however doubtful or ill-founded he might have considered them, or of opposing to them his own authority, destitute, as he probably was, of all personal experience, of the subject. He might also have thought, even if he had possessed more insight into the matter, than it is presumable he did, that, as the member of an orthodox body, it was fitting, that he should observe some decent regard to orthodoxy; and he probably was not unaware, that, it would not have tended to his personal comfort, to have unfolded, if it were in his power, a page of knowledge too copious for the condition of the times.

In all cases, therefore, he would have judged it more discreet, and becoming, under the circumstances described, to take such shelter, as they afforded, under the wings of Fracastorius, Alexander Benedictus, Forestus, Sennertus, &c.

than to appear himself in the character of an original authority, in contradiction to a junta of names still of formidable renown : and he might thus, without personal responsibility, serve as the ostensible medical authority, to justify the proceedings of the privy-council.

But nearly a hundred years have since elapsed ; the frauds and impositions of the 16th century are now fully unfolded ; and several of the obstacles to free enquiry, which prevailed proportionally in succeeding ages, have either wholly disappeared, or very much diminished in force. It is, therefore, allowable to presume, that, if Dr. Mead lived in the present day, his "Discourse" would have been differently conceived. But, whatever degree of superior authority may be attached to the name of this physician, it can only serve to render the refutation of the errors, to which he has lent its sanction, a more indispensable obligation upon the cultivators of medical science. It is in this view, and considering him as the representative of the advocates for contagion, and his book as containing the marrow of all that has been written upon that side of the question, that I have thought fit to say so much of the celebrated discourse of Dr. Mead.

During the whole of the intervening period, the doctrine of contagion has retained almost

undisturbed possession of the schools; has been uniformly acted upon by all Christian governments; and has never, since its first establishment, as far as I know, been called in question, in a regular way, as it regards epidemic diseases in general[1]: or, if called in question, it has never been satisfactorily refuted.

[1] Attempts to disprove contagion partially, are a virtual acknowledgment of its existence generally, in epidemic diseases.

## CHAP. XV.

*Contagion, in plague, questioned, in* 1758, *by Sir Richard Manningham, and, in* 1774, *by Dr. Maximilian Stole—Quarantine abolished and re-established in Austria—doctrine extended successively to typhus, yellow, jail, hospital, and ship-fevers, dysentery, and even scurvy—embodied into a regular system in Britain—Sir John Pringle and Dr. Huxham—typhus carcerum of Sauvage—Black assize at Oxford in* 1577—*Assize at Cambridge in* 1521—2—*Wood's Antiquities—Stow—Hollingshed—remarks on the assertions of Mead—Dr. Caius—Feb. Ephem. Brit.—Sickness at Taunton in* 1730—*at Launceston in* 1742—*Old Bailey Sessions in* 1750—*Assize at Exeter, in* 1586—*curious opinions of Dr. Bancroft—Dysentery considered contagious by Hildanus, Sennertus, and Sir John Pringle, &c. and Scurvy by G. Fabricius, and Sir Gilbert Blane—to doubt contagion has been considered foolish, or criminal—researches, sickness, and death, of the benevolent Howard.*

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This doctrine, as it respects plague, appears, to have been questioned, in 1758, by Sir Richard Manningham, a physician of London; and, in 1774, by Dr. Maximilian Stole of Vienna. But, besides that no circumstances existed, at the time of their writing, of sufficient

general interest to excite the public attention strongly toward the arguments, which they might have employed, they do not appear to have entered upon the subject with a view to any general, or extensive range of investigation, or with the expectation of obtaining any great or permanent results. The work of Sir Richard Manningham was calculated for the express purpose of obviating at home the calamities of an impending scarcity: and the labours of Dr. Stole were equally directed to the removal of a local grievance. In England, no particular consequence followed from the former; and, if, in Austria, the latter effected the temporary abolition of quarantine, it was shortly after restored, and the institution now exists in full vigour [1].

If any efforts, of even a partial nature, have been since made, in refutation of this hypothesis, they must have had results directly opposite to those which were intended: for we find, not

[1] Quarantine, which was then performed at Semlin, is now, as restored, performed on the frontiers of Transylvania, at a place, from its destination, called the Contumatz, near the Rothen Thurn, (Red Tower), on the direct road, through Wallacia, from Constantinople to Vienna. The term has been borrowed from the Italian, contumacia, in that language, signifying the state of separation, or seclusion, in which persons, *suspected* of being *infected* with the plague, are placed.

only, that the doctrine of contagion, with the trifling and partial interruptions, which I have mentioned, and perhaps some others, which have not fallen in my way, has maintained full dominion over the minds of the inhabitants of Christendom, throughout the eighteenth century; but that its sway has, in the course of that period, been even considerably extended. Originally limited to plague and scarlet fever, it afterwards successively embraced typhus, yellow, jail, hospital, and ship fevers, dysentery, and even scurvy.

The earliest traces, which we can find, of this doctrine, as applied to these last-mentioned diseases, are at the commencement of the eighteenth century. But it generally happened, at that period, that when an author wished to attribute any epidemic to a contagious origin, he called it the plague. It was not until an advanced period of the eighteenth century, that the doctrine, with respect to other fevers, dysentery, &c. was embodied into a regular system: and for this achievement, the world is principally indebted to the writers of our own country. At present, when foreign medical writers have occasion for authorities in support of contagion, especially in typhus, jail, prison, or hospital fevers, it is usual with them to quote Sir John Pringle, Dr. Hux-

ham, or some other British author, who has treated of camp or contagious diseases [1].

The sickness which arose during the black assize at Oxford, in July, 1577, so clearly depended upon noxious air, that, although it happened thirty years after the promulgation of the doctrine of contagion, it was not attributed to that source; excepting by the physicians, who fled [2], having most probably imbibed that papal doctrine in some of the Universities of Italy.

Upon this occasion, such only were affected, as had been actually exposed to the air. They all sickened a day or two after the third day of the assize, being the 6th of July; and were all dead, or recovered by the 12th of August: "after which died not any of that sickness, for one of them affected not another [3]."

Stow wrote in the early part of the seventeenth century. He would not have made this observation, if he had written before the middle of the sixteenth; *i. e.* before 1547.

[1] It is upon the authority of Huxham and Pringle, that Sauvages, has admitted a contagious disease, under the denomination of "Typhus Carcerum," into his Methodical Nosology.

[2] Wood's History and Antiquities of the University of Oxford, Vol. II. p. 188, et seq.

[3] Stow's Chronicle.

Sir Richard Baker says, " all that were then present, almost every one, within forty hours died, *except women and children.*" If women and children did not die, whilst others did, we may conclude that it was because they were not present at the trial. None however considered this sickness as arising from contagion. Some thought that it was devised by the Roman Catholics, who used the *art magic* in the design; the Register of Merton College, that it sprung " ex artificionis *diabolicis,* et plane *papisticis* flatibus e *Lovaniensi* barathero excitatis;" and others, that it was " *ingens miraculum,*" a judgment on the cruelty of the judge for the sentence passed upon the bookbinder.

In 1521—2, in Lent time, a similar circumstance happened at the assize at Cambridge. All who were present were taken sick; and many died [1]. Neither this, nor the disease called Sudor Anglicus, which appeared first in England, as is supposed, in 1485, nor any other fever was ever spoken of as contagious, till after the year 1547 [2]. If they were, I shall be glad to see the authorities.

[1] Wood's History, &c. Vol. II. p. 188.

[2] This was the year, in which the doctrine was promulgated by the authority of the Pope, and the Council of Trent. Fracastorius's book, to pave the way, had been published in 1546.

Mead says, that "the sweating sickness, called the Sudor Anglicus, and Febris Ephemera Britannica[1], because it was commonly thought to have taken its rise here, was probably of a foreign original," &c. "We learn from histories," he adds, "that the first time this was felt here, which was in the year 1485, it began in the army, with which King Henry VII. came from France, and landed in Wales[2]; and it was *then* thought to have been brought into France from the famous siege of Rhodes by the Turks, three or four years before[3]." "And of the four returns, which this has made since that time; viz. those in the years 1527, and 1528, *may very justly be suspected* to have been owing to the pestilence, which at those times raged in Italy, particularly at Florence and Naples[4]. And the others were *very probably* from a Turkish infection[5]."

Upon these vague statements of Dr. Mead, giving him entire credit for sincerity, I beg to observe, 1. That the name given to this disease

[1] The same fever was called, by Wedelius and Mayerne, Febris Hungarica. *Jos. Brown's Treatise of the Plague. London*, 1720. *Pref. Epist. p.* 9.

[2] Caius de febre ephemera Brittannica.

[3] Discourse, p. 6.

[4] Rondinelli Contagio in Firenze, and Summonte Histor. di Napoli.

[5] Discourse, p. 7.

at the time, sufficiently indicates the opinion *then* entertained of its origin. 2. Dr. Mead does not inform us *by whom* it was *then* thought to have been brought into France, from the siege of Rhodes; for the best possible reason, because the idea of an epidemic disease being propagated by contagion was not entertained by any one at that time. I beg to disclaim the intention of impeaching Dr. Mead's integrity. Not being aware of the existence of deception upon this point, he fell as naturally into the delusion, as Lord Bacon and Gibbon, as stated in the second chapter. 3. He quotes Caius (Keys) to shew that it was imported by the army of King Henry, which had landed from France in Wales. But Caius expressly gives it a domestic origin: "His (nostris Britannis) enim morbus hic ut adventitius non est, ita novus non est. Siquidem ex historiis Britannicis intelligo, *malum hoc ex Britanniâ ortum esse* (quantum scire licet) in exercitu Henrici Regis Angliæ ejus nominis septimi, quam *partim* ex Galliâ tum nuper secum adduxerat, *partim* in Walliâ propter Milfordium, quo classe appulit, conscripserat [1]."

Friend says, of the sweating sickness, that, "it began at first in 1483, in Henry the VIIth's army, upon his landing at Milford-Haven, and

[1] Caius de Feb. Ephem. Brit. new edit. p. 19.

spread itself in London, *from the 21st of September to the end of October.* It returned five times, and *always in summer:* first in 1485; then in 1506; afterwards in 1517; when it was so violent that it killed in the space of *three* hours; so that many of the nobility dy'd, and of the vulgar sort, in several towns, half often perished. It appeared the fourth time in 1528, and proved mortal then in the space of *six* hours: many of the courtiers dy'd of it, and Henry VIII. himself was in danger. In 1529, and only then, it infected the Netherlands and Germany; in which last country it did much mischief, and destroy'd many, and particularly was the occasion of interrupting a conference at Marpurgh, between Luther and Zuinglius, about the Eucharist. The last return of it with us was in 1551: in Westminster, it carried off 120 in a day, and the two sons of Charles Brandon, both Dukes of Suffolk, dy'd of it. At Shrewsbury particularly, where our author, Caius, resided, it broke out in a very furious manner. The description he gives of it is terrible, like the plague of Athens. He very properly calls it a *pestilent contagious* feaver, of *one natural day*[1]."

If Caius, as Dr. Friend says, calls this disease, upon its last return, in 1551, a *contagious* pesti-

[1] History of Physic, Vol. II. p. 333, 334.

lence, it is obvious where and when he must have imbibed this doctrine. But a contagious disease never kills in *six*, much less in *three* hours: and never commences and ceases at regular and determinate periods.

It must be unnecessary to comment upon his *suspicions* that the fevers of 1527 and 1528 *might* have been imported from Florence or Naples; or that those of subsequent years *might* have been imported from Turkey.

An instance of supposed jail *infection* is mentioned in the Gentleman's Magazine for May 1750, to have occurred during the Lent assize at Taunton, in the year 1730. And Dr. Huxham speaks of a fever, which happened at Launceston, in 1742, occasioning great mortality, which he supposes to have arisen from a contagion *generated* in the prisons, and disseminated by means of the county assize[1].

The next instance of this fever we hear of is in the Old Bailey sessions, in the spring of 1750, when the Lord Mayor, and two of the judges were affected. This was the fever which gave rise to the work of Sir John Pringle, already mentioned.

Between typhus, jail, hospital, and ship fever, I have here made no distinction, since they are all

[1] De aere, &c. Vol. II. p. 82.

produced by various modifications of noxious air; and are all as completely independant of contagion, as of any power, from the sphere of whose influence they are distant many thousand miles [1].

[1] Hollingshed, Vol. II. p. 1547, relates the history of a sickness which happened during the assize at Exeter, in March, 1586. Dr. Bancroft, at p. 661, of his *Essay*, says this "appears to have been the *true jail distemper*." What *he* considers the *true* jail distemper is one that is contagious; and he takes it for granted that *this* was contagious, because Hollingshed says, that "it was *dispersed* throughout the whole shire." In this expression doubtless Hollingshed was correct: for it could not but have been dispersed throughout the shire, inasmuch as the habitations of the different persons, who had been present at the assize, and were afterwards seized with sickness, might be scattered. Having proved the doctrine of contagion in epidemic diseases to be impossible, it cannot be necessary for me to enter into refutations of mere unfounded assertions, in regard to particular epidemics. But, if proof could now be obtained of this fact, I would pledge my existence it would be found, that none, but those that were actually present at the assize, were affected with the particular malady in question. The most ridiculous part is yet to come. "And, on this subject," says Dr. Bancroft, (p. 663, 664.) "the author makes an *important*, and I believe *just* observation, concerning the time which the infection remained *dormant in the system:* viz. 'It resteth for the most part *fourteene daies and upwards, by a secret infection, before it breake out into force and violence.*' This 'upwards' may mean the whole of a man's life, if it should be one of no ordinary duration. But the 'fourteene daies' constitute a period sufficiently extravagant, not to require any additional ground of disbelief,

With respect to dysentery, Hildanus, Sennertus, Sir John Pringle [1], and a thousand imitators, have considered it to depend upon contagion: and scurvy has been regarded by Fabricius, Sir Gilbert Blane, and perhaps by others, as arising from the same source.

By the middle of the 18th century, the catalogue of diseases reputed contagious was augmented prodigiously beyond its original amount. Thus, we see, by what degrees error may be perfected. The torrent was overwhelming and irresistible. If any one has dared even partially to call the doctrine in question, he has run the risk of being treated as an enemy of the human race. It was not sufficient for its advocates that they could safely issue their own opinions, under the double ægis of papal infallibility, and popular alarm; but they have charged all who presumed to differ with them, even in a small degree, with contumacy, when they have, upon any occasion, refused to receive their assertions as facts; or when they have declined to enter upon the hopeful task

I could as easily credit the capability of Alexander Benedictus's feather-bed to communicate contagion at the end of *seven years*. In a preceding chapter, I have shewn the absurdity of the various intervals assigned to infection."

[1] "This disorder" he says, "is not *so* catching as most others of *the contagious kind*." *Obs. on the diseases of the Army, last edit. p.* 235.

of proving a negative. They have even imputed to them unwarrantable motives. Thus, in our own times, the benevolent Howard has permitted himself to pronounce, for a philanthropist, rather a harsh censure upon the endeavours of Dr. Maximilian Stole, in procuring the abolition of the quarantine establishment, upon the Turkish frontiers, of the Austrian dominions, from the paternal administration of the Emperor Joseph [1].

In fairness, we are bound, until the contrary appears, to regard the motives of the advocates for each side, of any question, as equally disinterested: or, more properly, we have nothing to do with their motives, but only with the arguments, of the contending parties. If, however, we are to take undue biasses into the account, we ought to expect, in preference, to find them attached to that side of a question, which affords the best prospects of laws, regulations, and establishments; *i e.* which presents the most abundant crops of business.

In this instance, no such imputation can attach to the opponents of the doctrine of contagion, since the immediate consequence of the adoption of their principles, would be the abolition of quarantines, and every police institution, connected with that opinion. And it would be

[1] Lazarettos, p. 42.

somewhat monstrous to suspect them of wishing gratuitously, or for sport, or even for the sake of an ephemeral triumph in argument, to induce governments to pursue a course, which might increase sickness and mortality amongst their subjects. For my part, I shall only say of myself on this topic, and perhaps it ought to be admitted as some proof of sincerity, that there is no measure in my contemplation to propose, of which I shall not willingly be the first personally to incur the danger. On the other hand, I should consider it the highest presumption to deny, to the advocates for the opposite opinions, the credit of equal sincerity. With respect to the illustrious Howard especially, although an excess of noble enthusiasm, and some mis-information, had led him rather hastily to charge the ingenious and respectable Dr. Stole with unworthy motives, it would be both illiberal and unjust to suspect himself of want of sincerity, or of any mean bias, or interested view.

But not so the executive agents of the plague institutions, in the Levant, from whom he procured the principal part of his information, and to whose evidence the authority of his name has unfortunately given a weight, that does not belong to it: persons, who, if they could be supposed to be unbiassed by interest, the very existence of their situation depending upon the

duration of the popular belief in contagion, are almost necessarily confirmed in error, by habit; who, in all their answers to his queries, evince, that, upon the whole of the subject, they have no fixed or uniform opinions, but merely a vacillating belief; and whose statements consist, not of facts established by proofs, but of vague and unsupported assertions, the testimony of every one being in contradiction to that of every other, and each inconsistent with himself[1].

On this part of the subject, I should not have thought it necessary to say so much, did it not appear, that, upon the delusive information, obtained by Howard, from these impure sources, has principally been founded the decision of the British government, respecting the expediency of establishing lazarettos in England; and that it has unhappily contributed to procure a farther respite for the pernicious doctrine of contagion, and all its baleful consequences.

As, in the course of this discussion, the fallaciousness of that evidence is exposed, under the heads, to which its several parts respectively belong, it would be superfluous to enter here into any regular examination of it, as a whole.

[1] Howard's queries, and the answers of some physicians of the Levant, in his work upon Lazarettos, p. 32. et seq.

But, of Howard himself, who seems to be, upon this subject, the first person who has deserved the name of an investigator, it is necessary that I should say more. This excellent man, having set out with so decided a belief in contagion, that he appears to have thought it foolish, or criminal, to doubt, and taking it of course for granted, that lazarettos are well calculated for their intended purposes, directed his researches towards discovering their defects, and the means of improving them.

In the course of this investigation, he was unfortunately seized with a fever, at Cherson, of which he there died. The account of his last illness, as given by Dr. Clarke [1], evinces that his premature death was occasioned by the consequences of a double error; first, the belief that the disease, under which he laboured, obviously a fever of the most ordinary kind, occasioned by fatigue and moisture, was produced by contagion: and secondly, that negation was the proper mode of treatment.

"Mr. Howard was sent for to visit a lady, who was dangerously ill. She lived about twenty-four miles (thirty-five versts) from Cherson. He found her in imminent danger, he prescribed what he thought proper, and desired her family

[1] Travels in Russia and the Crimea, Vol. II. p. 604.

to send again for him, if she got better, adding that if, as he much feared, she should get worse, it would be to no purpose. Sometime after his return to Cherson, a letter arrived, stating the lady was better, and begging that he would come without loss of time: when he examined the date of the letter, he perceived that, by some unaccountable delay, it had been eight days in getting to his hands. Upon this, he resolved to go with all possible expedition. The weather was extremely tempestuous, and very cold, it being late in the year, and the rain fell in torrents. In his impatience to set out, a conveyance not being immediately ready, he mounted an old dray horse, used in Admiral Mordvinof's family to carry water, and thus proceeded to visit his patient. Upon his arrival he found the lady dying; this, added to the fatigue of the journey, affected him so much, that it brought on a fever. His clothes, at the same time, had been wet through; but he attributed his fever entirely to another cause. Having administered something to his patient, to excite perspiration; as soon as the symptoms of it appeared, he put his hand beneath the bed-clothes to feel her pulse, that she might not be chilled by removing them, and believed that her fever was thus communicated to him. After this painful journey, Mr. H. returned to Cherson, and the lady died. Failing

to pay his daily visit to Admiral Priestman, that gentleman called upon him, and found him weak and ill, sitting before a stove, in his bed-room. Having enquired after his health, Mr. H. replied that his end was approaching very fast, that he had several things to say to him, and thanked him for calling. The Admiral, imagining the whole to be the result of low spirits, tried to change the conversation. Mr. H. soon assured him it was otherwise, that he contemplated his dissolution with cheerfulness, for death had no terrors for him. He added that he had but a short time to live, and that his mode of life rendered it impossible that he should get rid of the fever, that as he had existed upon vegetables and water, and a little bread and tea for years, he could not lower his diet, and therefore believed he must die. He fixed on the spot, where he wished to be buried, (near the village of Dauphigny) five versts from Cherson, on the road to Nicholaef. He begged Admiral Priestman to secure this spot, and desired to be interred without any pomp. He became delirious previous to his death, and long persisted in rejecting medical advice. At last, at the earnest solicitation of Admiral Mordvinof, he allowed a physician to be sent for, who administered the musk draught, which is used in Russia, only in the last extremity. Admiral M. gave it him. He swallowed a little, but endea-

voured to avoid the rest, and gave evident signs of disapprobation. He shortly after breathed his last. These particulars of Mr. H's. death, were communicated by his two friends, Admiral Mordvinof, then chief Admiral of the black sea fleet, and Admiral Priestman, an English officer in the Russian service."

As the death of Lycurgus was calculated to give perpetuity to his laws, so the death of Howard might, if it had stood in need of such support, have contributed to prolong the belief in contagion. But we should not, in matters of science, allow ourselves to be dazzled by the splendor of philanthropic views, which, notwithstanding the most noble intentions that can animate the mind of man, may often be productive of consequences the most pernicious to society. This observation, I fear, will apply to many of the well intended efforts of Howard. But it is only with the effects of that generous zeal, which he employed upon epidemic diseases and lazarettos, that I have here any concern. In these respects, it will follow, as a direct consequence, from my principles, that the whole of his views were mistaken: and that, had they had any effect in confirming, or prolonging, the belief in contagion, or in enforcing the expediency of quarantine and lazarettos, his efforts would have been in so far pernicious.

The fact is, however, that the researches of Howard have effected no change whatever, in the state of opinions, or knowledge, respecting epidemic diseases. They have left the matter precisely where they found it; with the sole difference of having occasioned the transfer of quarantine for British ships, from the Mediterranean to England, and the establishment, in the latter, of lazarettos: *i. e.* a more complete extension to this country of the evils, which had previously been entailed, by the doctrine of contagion, upon the nations of the continent.

It appears, then, that, whilst those who might not have been able to give full credit to this delusion, would have found it a service of extreme danger to have proclaimed, even in a partial manner, their dissent; the principal labours of the adherents of this hypothesis, no proof of its truth being deemed necessary, have, from its first promulgation to the latter end of the 18th century, consisted in vain endeavours to reconcile contradictions. After having "espoused this well endowed opinion," either because it was in fashion, because it had emanated from a source which was regarded sacred and infallible, or because the examination into its merits would have been a troublesome enquiry, its partisans have been successively occupied, for two hundred and seventy years, in seeking

arguments, (in the words of Locke, which I have chosen for the motto of this and the preceding chapter) "to varnish its deformity."

With the present century has arisen a more ardent spirit of enquiry. Practical researches, concerning epidemic diseases, founded upon principles, which I had, towards the conclusion of the last century, the good fortune to trace, and the boldness, or perhaps the audacity, to promulgate, have been attempted, by several persons, since the commencement of this century, although unhappily without success: and the additional good fortune seems to have been reserved for myself of being the first to obtain, in the highest species of epidemic, practical elucidation, both in respect to the cause, and to the cure, of the truth of my original conclusions.

An account of these researches will form the first part of the succeeding volume.

## CHAP. XVI.

*Observations on a late publication of Mr. Pym—absurdity of the idea of exterminating epidemic diseases—delusive and pernicious doctrines, and nomenclature, of yellow fever—errors of Dr. Bancroft, respecting typhus and plague—if these diseases were contagious, they would not manifest a preference for particular countries.—Small-pox has no fixed head-quarters.*

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In attempting to sum up the evidence, which has been adduced in the preceding chapters, I feel that the subject is of an importance extremely difficult to be conceived, and, by my humble powers, quite impossible to be adequately represented. But it does not follow, that, we should not attempt to develope an important subject, because we are unable to do full justice to its merits.

It is curious to contemplate the different or opposite nature of the means, which different persons will pursue, in professing, and probably really intending, the same ends. "With a view of preventing a recurrence of the miseries and mortality, which have been produced in different

parts of the world, in consequence of erroneous opinions respecting the nature of yellow fever," Mr. Pym [1] writes a book to prove, as he says, that it is a highly contagious disease; that the contagion, upon which it depends, is a native of Bulam, on the coast of Africa; that it is now, as it has been for a long time past, performing a tour of the earth, for the express purpose of propagating its sable race; and, finally, he proposes, by means of quarantine, lazarettos, and medical police, nothing less than its total extermination [2]!

These marvellous institutions he of course thinks, although he has omitted to recommend it in express terms, should be multiplied throughout the universe, wherever this African monster is likely to shew his merciless face. "Having been more than once witness to the dreadful ravages committed by this disease," says Mr. Pym, "and having for several years *acted successfully against its influence,* I have thought it a duty incumbent upon me to step forward *in the cause of humanity,* with the view of combating the errors and opinions relating to it, which have been so *industriously circulated, even by medical men, who have never seen it;* and by *a*

[1] Observations, &c. Dedication to the medical officers of the navy and army. [2] Ibid. pref. p. 5.

*collection of facts and arguments,* to convince *the most prejudiced,* that the disease in question is *highly contagious ;* and that *its mortality may be prevented by the establishment of quarantine and a good police*[1]."

Since the conflict of opinions cannot fail ultimately to elicit the truth ; and discussion, to prove a more powerful antagonist of pestilence, than quarantine, we ought to hold ourselves indebted to Mr. Pym, for having, even although it may not be wholly in the quality of a volunteer, engaged in the controversy.

This writer, as I collect from the title-page of his work, is a Deputy Inspector of Hospitals, and late *a Superintendant of Quarantine,* and President of the Board of Health, at Malta : and in these capacities, the public would naturally have ascribed to him a certain portion of authority, in respect to pestilential diseases. But, however we might be disposed to defer to such an authority, in all things reasonable, it will scarcely be expected, that, in a matter of such extraordinary importance, we should be ready to subscribe to the opinions of any person, however little fallible, or however much experienced, without examination. Let us cursorily examine those of Mr. Pym.

[1] Pym's Obs. Pref. p. 12.

He sets out by stating, that he has "for several years *acted successfully against the influence* of yellow fever;" but he omits to inform us, when, where, or how. Was it at Gibraltar, or at Malta? Was it by quarantine, lazaretto, and medical police establishments? Or does he seriously mean to impress us with the belief, that, when pestilences have *not* occurred, they have been *prevented* by these institutions; and, that, when they *have occurred*, it has been *because the police has not exercised its usual vigilance?*

The friends of free discussion can have no desire to withhold from Mr. Pym the credit that may be thought due to him, for having stepped forward, as he says, "in the cause of humanity;" provided they themselves are not accused, in preferring opposite measures, of wishing to destroy mankind. When he talks of errors circulated with industry, by persons, who never saw the disease, does he mean to insinuate, that there are insane persons going about the world, gratuitously, to circulate error, knowing it to be such, and with the intention of injuring their species; or to recommend implicit deference to the authority of plague guardians, and superintendants of quarantine, or to deprecate all enquiry? Alarm has been felt, or feigned, in various quarters, on account of the attempts

which have been made to disturb the venerable doctrine of contagion. But, unless doctrines could at once be true, and capable of being refuted, I am unable to comprehend how truth can ever be injured by discussion. The opponents of the doctrine of contagion, do not address themselves to the passions, prejudices, or presumed interests, of the multitude, high, or low; but to the cool, and dispassionate reason of the reflecting part of the community. Nor ought they to be censured, if they should be unable to yield to the force of that "*collection of facts and arguments,*" by which Mr. Pym has expected "to convince *the most prejudiced*, that the disease in question is *highly contagious;*" notwithstanding his notable discovery of certain "*peculiarities* belonging to it, which were unknown before, *particularly that of its attacking the human frame but once*[1]."

As, when epidemic diseases have not occurred, they have been supposed to have been prevented, and when they have ceased, to have been extinguished, by the exertions of the police institutions; so, when persons have remained exempt from a second attack of pestilence, it has been inferred that the disease is *incapable* of affecting the same person a second time!

[1] Pym's Obs. Pref. p. 7. See chapter vii.

Thus, by confounding the *principle* of the incapability of recurrence of the disease, with the *fact* of its non-recurrence, in the same person, an attempt is made to lay the foundation of a fresh superstructure of error. But, if this alledged "peculiarity" were admitted, in its fullest extent, it could prove nothing. It would be necessary to shew, besides, that the disease does not appear and cease, at certain determinate periods, on the coast of Africa, in America and the West Indies, and in Spain; before its dependence upon contagion could be admitted to be possible.

This circumstance (the alledged fact of its affecting the human frame but once) Mr. Pym considers "of the utmost consequence, not only as relating to the *comfort of the sick,* but as assisting materially in the *regulations necessary to be established,* with the view of *checking the progress of,* or *totally exterminating* this MOST TERRIBLE OF ALL DISEASES[1]."

Immense, or almost incalculable, as are the interests, which this enquiry involves, it is not a little extraordinary, or rather it is hardly credible, that, upon a strict investigation, the real bearings of the subject, upon the welfare of nations, should prove to be in a sense, not simply

[1] Pym's Obs. Pref. p. 7.

different from, but diametrically opposite to, that, in which it has been held for the last 270 years.

It is for objects precisely similar with those which Mr. Pym professes to have in view, in recommending farther establishments of quarantine and medical police, that *I* recommend the abolition of those institutions, where they already exist: viz. in order to prevent the recurrence of the incalculable sickness and mortality, which have, for the last two hundred and seventy years, been produced by epidemic diseases, throughout the world, in consequence of the delusive opinions, which have, during that period, amongst Christians, almost universally prevailed, both respecting their cause and prevention, and their nature and cure; as well as in order to obviate, or to remove numerous other evils, which have been entailed by these opinions upon society.

But, in his project of "exterminating this most terrible of all diseases," Mr. Pym soars many degrees beyond the limits, which I had affixed to my comparatively sober expectations. Content if I should have succeeded in ascertaining and indicating to a certain degree the means, by which epidemic maladies may be prevented, mitigated, and cured, I shall not, until men have learnt to live without air, or until air

has ceased to be capable of becoming noxious, turn my views to the means of their extermination.

Whilst our means are thus of a diametrically opposite nature, it is fair to presume, that, as Mr. Pym, in advising the multiplication of plague police institutions, can have no prepossessions, independently of his belief in their utility, in favour of those establishments; so, in recommending the abolition of such as already exist, I can entertain no prejudices against them, independently of my conviction of their highly pernicious operation.

With respect to doctrines, I may observe, that Mr. Pym, in common with the other advocates for the hypothesis of contagion, as the cause of epidemic diseases, have found themselves under the necessity of attempting to reconcile all sorts of contradictions. Thus, we are gravely informed, its existence being as usual assumed, that "The contagious powers of *typhus* are *destroyed* by heat, those of the *Bulam fever* are *increased* by it[1]." It seems, then, that heat both *invigorates* and *destroys* contagion! Admitting for a moment that this contradiction could take place, it would follow, that we might cure, and extirpate typhus, simply by the application

[1] Pym, p. 201.

of heat, and Bulam fever, simply by the application of cold. But none of these circumstances happen in nature; and after we have perused a whole volume, we find ourselves to have been reasoning, concerning a nonentity. Having set out on a road, diametrically opposite to the right one, what chance have we ever to get to our journey's end, but by returning to the spot from whence we set out, and taking a new departure? It now seems that this contagion, after all, is, even by the confession of its partisans, nothing of itself, but as it is affected by some one of the elements.

"A free circulation of air alone, or particular states of the air, have *wonderful influence* over *different contagions*[1]." Wonderful philosophy!

"The *Bulam fever, for instance,* does not exist in the cool airy situations *in the mountains of Jamaica,* where the thermometer ranges from 50 to 70[2]." What fever *does* dare to shew its face in those regions? But, if the small-pox be carried there, will not the disease be propagated, as freely, as in the most impure atmosphere? It is quite obvious, why a disease, which depends upon a noxious state of the air, and *not* upon contagion, should never appear, where the air is pure; and what seems now so very wonderful to

[1] Pym. p. 201. [2] Ibid.

Mr. Pym, would not be at all surprising, if he could expel the idea of contagion from his mind.

One might marvel, if it were not a phænomenon of such frequent occurrence how men can continue to bewilder themselves, and to confound reason and common sense at such a rate, nay to shut their eyes against the light of truth, as conveyed, without their intending it, almost by their own words. Let us try how small an alteration of language will make sense of nonsense. By simply substituting "*diseases*' for "*contagions,*" we effect the metamorphosis. Thus: the sentence "A free circulation of air alone, or particular states of the air, have wonderful influence over different *diseases,*" although it would not comprehend much meaning, would, in this case, convey no delusion.

"The contagion of typhus," he goes on to say, "is condensed, or rendered more powerful (has it ever been seen, or its density ascertained?) by states of the air connected with cold, or moisture, and is dissipated, and rendered mild, by heat, and drought; in Bulam fever, it is the reverse[1]." If, by *Bulam fever,* which he might with as much propriety have called a *Japan*

[1] Pym, p. 202.

*fever*, he means that modification of fever, which depends upon heat and drought, in contradistinction to the modification, which depends upon cold and moisture, and which may be called *typhus*, if he chooses, I can understand him. But, when he comes in with the machinery of contagion, to explain, what is quite intelligible without it, he only becomes incomprehensible to me, and I should much doubt his being very clear to himself.

"The ravages committed by typhus (which it seems is not of the illustrious African, but of an European, contagious family), *in close and ill-ventilated apartments*, are known to be dreadful; when *little*, or *no* danger (of *infection*, I presume) is to be apprehended from patients, labouring under the same disease, *when removed to an open airy situation*[1]." Now, I could much more readily comprehend a proposition like this: "The foul air of close and ill-ventilated apartments, a loathsome prison, for instance, will produce a fever amongst its inhabitants: but persons in pure air will remain well, although those so affected should come amongst them: consequently it cannot depend upon contagion:" This to my understanding, is perfectly clear: but mix it with contagion, and it

[1] Pym, p. 202.

immediately becomes unintelligible. Mr. Pym, however, seems to think, that, without a due admixture of contagion, truth itself must become error. "Dr. Haygarth," he proceeds, "has ascertained, *by experience* (strange!) that the *poison* of typhus *infects (poison generally kills)* twenty-two out of twenty-three persons, exposed to it (did the doctor, with a microscope, see the poison applied?) *in close, dirty, small rooms;* and that, *in clean, airy, and spacious chambers,* few, or none, are *infected.*" Here we have a perfect specimen of the loose jargon of the medical schools. Without the assistance of contagion in epidemic diseases, we should be in danger of losing the charter of our craft and mystery.

The precision of the Doctor, in respect to the proportional number of the affected, is admirable. In speaking so confidently, he has, it may be presumed, ascertained, *by experience,* what precise degrees of intensity of the *poison,* and of the closeness, dirtiness, and smallness of the rooms, are required, to *infect* twenty-two, or only five, ten, fifteen, twenty, out of the twenty-three original inhabitants; what are the circumstances, under which, *few,* or *none* are *infected;* as well as those, under which contagion can produce disease, without the help of the air; or the air, without the help of contagion. According to the learned Doctor's own account, the air seems

to play a much higher part, upon the epidemic stage, than the *poison* of *contagion*.

Let us hear what Dr. Bancroft says upon the same subject.

"Cold, however, is now certainly known not to produce any such effect, in regard to the *contagion* of jail or typhus fever, which, as has been already stated, is equally unknown in the habitations of the Russian peasants [1]."

If these diseases were contagious, would they not oftener travel? Is not the constant intercourse between all the nations of the globe, sufficient to admit of an interchange of contagions? If they depended upon contagion, would plague remain in Turkey; and typhus fix its abode in England? If contagion could entertain a preference for particular countries, and if these diseases depended upon contagion, are there not others that the plague might deem more congenial than Turkey, and that typhus might deem more congenial than England? Or, having no preference, would it not regard all soils alike? Does small-pox fix its head-quarters in any particular spot, from which to make only occasional excursions?

I cannot refrain from offering to the reader as relevant to the present purpose, one of the most

[1] Essay, p. 149.

exquisite, as well as most correct specimens of medical speculation, which I have any where met with. It is from the pen of the same writer, who amuses himself by enquiring, what might be the *possible* consequences of the *co-existence* of two *non-entitities* in the living body. But, lest I should not do justice to this brilliant conception, I shall relate it in the author's words:

"It may indeed be possible that a person who has been exposed to the contagion, both of plague and typhus, should be attacked by both diseases together: but, in this case, the infection of each would doubtless remain distinct, and only be able to propagate its peculiar disease; *because* one is communicable by *immediate contact* only, and the other, *so far as we can judge,* exclusively through the *medium of the atmosphere* [1]."

It is a circumstance, which of itself ought to be sufficient to discredit the alledged influence of any agent, that its advocates cannot condescend upon the mode in which it exercises that influence. With respect to the plague, some affirm that it can only be conveyed by the breath, some by the touch, and some by the clothes. Bonaparte, having found that it was not communicable by the touch, inferred, having pre-

[1] Essays, p. 561.

viously taken contagion for granted, that it *must* be produced by the breath[1]. Those, who approach the breath, but are not affected, conclude that it *must* be occasioned by the touch; and those, who undergo both ordeals, without being attacked with the malady, conclude, with equal reason, that it is only communicable by the clothes. But, if all the three modes are tried, without the expected effect following, then the inference is, that the person exposed has, for the time, lost his susceptibility to contagion!

[1] Warden's Letters from St. Helena, p. 179.

## CHAP. XVII.

*The experience alledged to exist in epidemic diseases worsethan useless—under a system of delusion, its result is but improvement in the practice of an error—the inexperience of the mere novice less injurious—obligations to the works of Mr. Pym, Dr. Bancroft, Sir James Fellowes, Dr. Burnet and others—recapitulation of proofs.—Miscellaneous facts and observations.*

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It may be proper here to say a few words respecting authority, and presumed experience, in epidemic diseases. If the principles maintained in this work, approach in any tolerable degree toward correctness, it inevitably follows that all the experience, which is presumed to exist upon the subject, can consist of nothing more than a dexterity in the application of false knowledge. The habit of directing, or superintending establishments, whose regulations are founded in delusion, can confer no experience. Under such a system, all acquired dexterity, is but improvement in the practice of an error; and must, in its exercise, be more injurious to society, than the inexperience of the mere novice.

I trust these observations may have set the matter of authority, and alledged experience, in pestilential diseases, in its true point of view; and that the reader is prepared to receive, upon the ground of their intrinsic merit, all the facts and arguments, that may be adduced, upon either side of this interesting and deeply important question.

Having thus, as I think, destroyed the main defences recently erected by Mr. Pym, Dr. Bancroft, Sir James Fellowes [1], and others, in the untenable fortress of contagion, I proceed to take a cursory review of what has been effected in the preceding chapters, and to supply omissions, and deficiencies of illustration.

In disproving the extraordinary but important hypothesis, that epidemic diseases depend upon contagion, I have thought it necessary, in order to obviate the prejudices founded upon the belief in the great antiquity of that doctrine, to begin by

[1] To these gentlemen, and to Dr. Burnet, the public are greatly indebted, for the valuable information, which they have collected and promulgated, upon this important subject, and the new truths, which their discussions have elicited. For myself, I must more especially acknowledge my particular obligations to the work of Sir James Fellowes; whose *facts*, unaffected by the leaven of his *opinions* (no ordinary merit), appear to be entitled to much more confidence than is due to medical reports in general.

shewing, that it was unknown to Hippocrates and the ancient physicians, and that it had not entered into the superstitions even of the vulgar of any nation, previous to the middle of the sixteenth century. These assertions are proved, by the nature of the doctrines, which were actually maintained by physicians previous to that period; by no precautions having been adopted, such as would necessarily result from the belief in that doctrine, had it been, at any time, entertained; as well as by a total absence of positive proof.

The second step in this process, was to trace and shew the occasion, upon which this curious and highly important doctrine was first introduced into the world: and in this, I trust, I have succeeded, to the satisfaction of the most scrupulous reader, in some of the preceding chapters. It there appears, that, with respect to plague, this doctrine was first published, in 1546, by Fracastorius, physician to the Council of Trent; that the dread of a contagious malady was, in 1547, made the pretext for removing that Council to Bologna; and that this doctrine, thus promulgated by the decree of a general Council, and sanctioned by the head of the Church, was implicitly received throughout the Christian world. The manner in which it was afterwards extended to other epidemic diseases, has also been explained in those chapters.

But, if, in order to remove prejudices, I have deemed it right to enter into these preliminary illustrations, it is not that I consider it necessary for me to give them any weight towards establishing my conclusions. As epidemic diseases undoubtedly existed amongst the ancients, as they have since done, and now do, and will, whilst air continues to be necessary to the existence of man, and to be occasionally noxious, continue always to do; and as it would as certainly have been impossible for contagion to have existed, as the cause of these maladies, without its having been discerned by physicians of former times; this consideration ought of itself to be decisive of the question, unless we suppose, either that the Pope, and the fathers of the Council of Trent, had organs better calculation to discover contagion, than Hippocrates, and the other enlightened physicians of his day; or, that, in respect to the cause of diseases, nature herself had, in the sixteenth century, undergone a revolution. But in order to obviate all cavil on this head, I shall consider it a matter of entire indifference, in respect to the truth or error of the doctrine, whether it had been universally believed, or universally disbelieved, or totally unknown, from the beginning of the world to the present day [1].

[1] The result of this enquiry may render it problematical whether it be not easier to discern contagion where it does not, than not to discern it, where it does exist.

Although, in as far as authority may be considered of any avail, that of Hippocrates and the ancient physicians will certainly be admitted to be of greater weight, in this question, than that of of Pope Paul III. the fathers of the Council of Trent, and all the writers, of whatever profession or calling, who have espoused the modern doctrine of contagion ; yet I wish not to avail myself of the preponderance, in favour of my argument, which this pre-eminence would confer. The question ought to be decided upon quite different grounds.

Nor will I insist upon the fact, that no proof has ever, in a single instance, been adduced, of an epidemic disease being communicated by contagion from one person to another ; from persons to goods ; or from goods again to persons. Upon this ground only, we should be quite justified in rejecting the doctrine ; since it is quite impossible, that, if those diseases depended upon so palpable a cause, proofs of it should not abound, sufficient to bring conviction from the most obdurate unbeliever. He who should call in question the contagious nature of small-pox, would only excite doubts as to the sanity of his intellects. When a question is considered undecided, the *onus probandi* is properly deemed to rest with those, who contend for the affirmative : But, on this occa-

sion, I can afford to waive this common privilege of disputation.

Viewing the question, then, entirely upon its own merits, I have entered into an investigation of the history of epidemic diseases, and the principal facts which relate to them; and, from these, have deduced the broad inference that they never depend upon contagion. From this examination. it results, that, if diseases, which are capable of affecting the same person repeatedly, could be communicated by contagion, they would never cease, where no precautions are taken, until communities were extinguished; that epidemic diseases are capable of affecting the same person repeatedly; and that they cease, where no precautions are taken, as in Turkey, without communities being extinguished: consequently that it is utterly impossible these diseases should depend upon contagion.

This alone might be received as sufficient demonstration. But the proofs are various, almost innumerable. The appearance, and cessation, of those diseases, at determinate periods: their progress: and the various liabilities of different countries, and persons, as described in the several chapters which relate to them, would also of themselves form a sufficient body of proof.

And I shall here only farther adduce some corroborative evidence, in the shape of miscellaneous

facts and observations, or such remarks as have been omitted under the heads to which they respectively belong.

Howard says, that "when the corpse is cold of a person dead of the plague, it does not infect the air with any noxious exhalations. This is so much believed in Turkey, that the people there are not afraid to handle such corpses. The Governor of the French Hospital in Smyrna told me, that in the last dreadful plague there, his house was rendered almost intolerable by an offensive scent (especially if he opened any of those windows which looked towards the great burying ground, where numbers every day were left unburied); but that it had no effect on the health either of himself, or his family. An opulent merchant in this city likewise told me, that he and his family had felt the same inconvenience, without any bad consequences [1]."

The Committee of Physicians of Moscow, in 1771, invented a fumigatory powder as a preventive against plague. Clothes, and even shirts of pestiferous patients, being prepared with this powder, were worn by seven criminals, who requested to undergo the trial. These clothes con-

[1] Lazarettos, p. 25, note. In the answers of the Proto-Medico, of Malta, to my queries, he hazards the assertion that infection may be received from dead bodies, so much is he determined that there should be contagion!

They had been worn by persons, who had died of the plague, and were impregnated with sweat, pus, and ichorous matter, from their bodies. The criminals were shut up, for a month, in an hospital, wearing these clothes; and they were not affected with disease. The inference deduced from this fact was in favour of the preservative virtues of the powder. It would have been infinitely less irrational to have concluded that no infection existed in the clothes.

During the great plague of London, in 1665, English ships from that port were freely received at Constantinople [1]; and, during the plague of Marseilles, in 1720, French ships from that port were freely received at Smyrna [2]: and they neither excited apprehension, nor communicated disease.

Men have been known to live with their wives, and women with their husbands, being afflicted with the plague, and to have perished of the disease, without communicating it to their partners.

The Rev. Mr. Dawes, in his account of the plague, at Aleppo [3], says: "among many partisisted of furs, woollens, cottons, silks, and thread.

[1] History of the Plague by H. F.

[2] Pye's Discourse, p. 41.

[3] Philos. Trans. 1763, No. xii.

culars, the following anecdotes seem somewhat extraordinary, but are well attested: last year, as well as this, there has been more than one instance of a woman's being delivered of an infected child, with plague sores on its body, though the mother herself has been entirely free from the distemper.

"A woman that suckled her own child of five months, was seized with a most severe plague, and died after a week's illness; but the child, though it sucked her, and lay in the same bed with her, during her whole disorder, escaped the infection.

"While the plague was making terrible ravages, in the Island of Cyprus, in the spring of 1760, a woman remarkably sanguine and corpulent, after losing her husband and two children, who died of the plague in her arms, made it her daily employment, from a principle of charity, to attend her sick neighbours that stood in need of her assistance, and yet escaped the infection. Also a Greek lad made it his business, for many weeks, to attend on the sick; to wash, dress, and bury the dead, and remained unhurt."

"A blacksmith, who worked at Carthagena, but whose residence was at some distance without the walls, contracted the disease, and died in the same bed with his wife; yet neither herself or their children were infected [1]."

[1] Burnet, p. 273.

Out of 4000 Spaniards removed to the neutral ground, those only had the fever, who were ill on going out, and they did not communicate the disease, to any of their neighbours or attendants [1].

Women remained perfectly healthy, who had been sitting on the beds, where men had been laying ill of fever [2].

Mr. Glasse says, "During the spring and autumn, I have been in the habit of seeing solitary cases of fever attended with *black vomiting*, and other severe symptoms, both in the town and south, without the disease, being communicated to others confined in the same building [3].

Mr. Whitmarsh, in 1813, in the course of his attendance on patients in the neutral grounds, was in frequent contact with the sick, and had the contents of their stomachs frequently thrown over him, yet remained in perfect health. But in 1814, when he resided in the naval hospital, *within the garrison*, he was attacked with the prevailing fever [4]."

Women suckling children, have had the disease, and died of it, without its being communicated to the infants; and children at the breast have had it, and perished, without the mothers being infected [5].

[1] Burnet, p. 325. [2] Id. 325. [3] Id. 329. [4] Id. 332.
[5] Sir James Fellowes, p. 484.

The carriers of the dead, and the nurses of the sick, are not more liable to the disease, if they be not strangers to the climate, than other descriptions of persons. The case of John Hayward and his wife, who were employed the whole time of the pestilence of 1665, in London, are here in point[1]; the latter to nurse the sick, and the former to carry the dead; and both continued in perfect health.

The carriers of the dead, however, must, from the fatigue, and nature of the service, the stench being murderous, independently of all ideas of contagion, be generally more liable to perish, cæteris paribus, than other members of the community. This is well exemplified by a curious and instructive fact, relating to the plague of Marseilles. When the brave sheriffs of that city, and the intrepid Chevalier Rose, headed the brigades of galley slaves, and the divisions of soldiers, who were employed to remove the dead bodies, amidst desolation and putrefaction, the former escaped unhurt, whilst the forty soldiers, who accompanied them, all perished, excepting four. This difference may

[1] *History of the Plague of London in* 1665, *by H. F. p.* 104, 5. There is also a remakable case of a Mr. Hare, who withstood the attack of a virulent pestilence on the Coast of Africa, as related by Howard, at p. 42, note, of his work upon Lazarettos.

be accounted for not simply by difference of living and labour, but principally perhaps by moral influence, fear of an invisible danger acting so much more powerfully on the untutored soldiers, than on the enlightened and high-minded sheriffs, and the gallant chevalier [1]. If contagion were the cause, the danger would be equal, uniform, inevitable: differing only in degree, according to the intensity of the operation of the virus, and the previous excitement of the patients, respectively. Terror can never accelerate the attack of contagion, although it will increase the noxiousness of its effects. But in an epidemic season, it is the powerful advanced guard of a deleterious atmosphere. The principles, upon which these effects depend, are explained, in examining the cause of epidemic diseases.

Persons employed in the Lazarettos to expurgate goods, although not the most salubrious occupation, have certainly not been oftener, but probably seldomer attacked with epidemic diseases than the generality of persons; which could not have happened, if they depended upon a contagion, capable of being conveyed by goods.

[1] "A Journal of what passed in the city of Marseilles, whilst it was afflicted with the plague, in the year 1720, under date the 8th of September."

"The fever of Carthagena ceased on the 23d of January 1805, on which day the last patient was received into the Royal Hospital. On the 5th of February following, a French frigate, having on board the wounded of the Arrow sloop of war, which she had captured a few days before, after a gallant resistance, arrived in the harbour; the wounded were immediately landed, and *placed in the same bed and bedding, in which several patients, who had died of the prevailing fever, had lain, without the bed or beddiug having been either washed, or aired;* YET NOT ONE OF THESE MEN WAS ATTACKED WITH THE DISEASE[1]."

Mr. Howard[2] gives an instance of the clothes of infected persons being worn by others, without their being affected. He expressly declares that the disease is not capable of being communicated by the touch[3]. Bonaparte does the same. Mr. Pym[4] gives an instance of contact with rags, which had been used by pestiferous persons, without the disease being produced. And of persons removed not infecting others[5]. The flannels used by pestiferous patients, at Marseilles, were preserved for the use of succeeding patients, in the hospitals[6].

[1] Burnet, p. 273. [2] Lazarettos, p. 46. [3] Ibid. p. 24.
[4] Pym, p. 196. [5] Id. p. 199, 200. [6] Lazarettos.

It is notorious that the clothes of the pestiferous persons, who die in the plague depots of the Levant, are regularly exposed to sale in the markets; that they are constantly in a state of transfer from the dead to the living. And, hence, if the disease were contagious, the infection, in all the towns of the Turkish Empire, would be constantly in a state of circulation, and the disease being capable of affecting the same person repeatedly, would never cease, whilst an individual remained alive.

Contagion, as a source of fever, is entirely rejected by the professional men, who have the greatest opportunity of information, now resident in the West Indies [1].

In London, in 1665, the people who came from the country with articles of consumption to market were not affected with the disease [2].

"The plague, in the year 1636, began with great violence; but leave being given by the king's authority, for people to quit their houses, it was observed that not one in twenty of the well persons removed fell sick, nor one in ten of the sick died [3]."

There are, in all places, some spots which

[1] Dr. Macarthur on the Diseases of Barbadoes, Med. Obs. Vol. VII. p. 325.

[2] History of the Plague, by H. F.

[3] Discourse upon the Air, by Thomas Lock.

usually preserve the purity of their atmosphere to such a degree, as not to be capable of occasioning disease, even in the most unhealthy seasons. Of this description is Sasi [1], on the Island of Malta; and Essouan [2], in Egypt. In London, during the plague of 1665, there were four parishes, in which there was no sickness [3].

"An old barber doctor, in Cairo, died in the year 1801, from plague, at the advanced age of 96 years. This man had been long celebrated among pestiferous patients, attended and bled them occasionally, and, at the age of 96 years, *caught*, for the first time, the *infection*, under which he sunk [4]." It seems, then, a man may be exposed for 60 or 70 years to infection, without being *infected*, and *catch* the disease at last! *Credat Judæus Apella!*

[1] In the plague which ravaged Malta in 1676, one village remained free from the distemper, and is therefore called Sasi, which signifies *pure*. *Boisgelin's History of Malta, p.* 56.

[2] "Essouan, the most delightful spot in Upper Egypt, and which has the singular advantage of never being visited by the plague, a privilege for which it is indebted to the mildness of its temperature, and the prevalence of strong northerly winds; though the inhabitants superstitiously ascribe it to the benign influence of a sheik buried in the neighbouring mountains." *Legh's Narrative of a Journey in Egypt, &c. p.* 5.

[3] City Remembrancer, Vol. II. p. 374.

[4] Whittman, p. 520.

The measure of shutting themselves up in their houses, which has been adopted by affluent strangers residing in the Levant, has been generally regarded, and I believe with truth, as tending to security from pestilence. The persons who are thus enabled to retire, are such as have their residence in the most salubrious parts of towns, in air of a much better quality, than that to which they might be exposed in travelling abroad; and such as are enabled, by their circumstances, to live in a suitable manner. But their exemption must depend principally upon the situation of the houses in which they are shut up. And the precaution has frequently been known, as will always happen when the houses shut up are in the midst of noxious air, to fail.

During the plague at Malta, those who shut themselves up in their houses, and preserved a strict quarantine, were in general preserved from the plague; but no such immunity was enjoyed by the inhabitants of Gibraltar. "Mr. Keeling, Mr. Lindblad, and Mr. Morison, respectable merchants, residing in Irish town, on the first alarm of fever, placed themselves and families in strict quarantine; yet they were all attacked with the disease. A Mr. Jacks, and his wife, who had retired to a place called Innes's farm, situated more than two thirds of the way

up the rock, and at a considerable distance from any other house, some time before the fever made its appearance, and being supplied with every necessary on the first alarm, placed themselves in strict quarantine; yet they were both attacked with the disease, and perished [1]."

"The fever attacked persons in different parts of the town at the same time, very remotely situated from each other; and those, who shut themselves up, and who may be considered to have placed themselves in quarantine, perfectly insulated, were attacked as readily, as those who mixed indiscriminately with the people [2]."

A lady, an officer's wife, residing in the Moorish castle, which is considerably above and out of the town, never left that place, and was so alarmed that she would not allow any individual to approach her; and after the fever had existed five weeks, or more, she was attacked, and died. *Her husband, who was constantly at her bed side, during her illness, escaped the disease* [3]."

"Similar precautions, Mr. Humphreys says, had been taken by the ordnance storekeeper, R. Pringle, Esq., *who had adopted the most rigid quarantine, for more than three*

[1] Burnet, p. 322. [2] Ibid. p. 346.
[3] Ibid. p. 347.

*weeks,* YET HE DID NOT ESCAPE. *Mrs. P. who was constantly with him, escaped, as did the children* [1]."

A vain attempt has been made to decide the question respecting the cause of the epidemic of Gibraltar, by the authority of numbers. Mr. Pym alledges that, all the medical faculty at Gibraltar, although at first, with the exception of Mr. Kenning, of a contrary opinion, were at last, with the exception of Dr. Nooth, unanimously in favour of contagion [2]. This could prove nothing, if it were correct. But Dr. Burnet gives a statement of the opinions of the medical officers of that garrison, at the conclusion of the epidemic of 1814, by which it appears, that of seventeen, eight were against contagion, seven in its favour, and two neutral [3]. I mention this as a matter of no farther importance, than as it indicates an increasing spirit of research, and the progress of a change of opinions, in respect to epidemic diseases.

It is well worthy of remark, that, of the twenty-three towns of Spain, which were afflicted with fever in 1804, in eleven, the greatest number of deaths happened on some day from the 9th to the 15th of October inclusively, to

[1] Burnet, p. 347, 348. [2] Pym's Obs. p. 23.
[3] Burnet, p. 439, 40.

which we may add Gibraltar, making twelve, or more than one half. In five more the greatest number of deaths happened also in October; in two in September; and the remaining five in November[1]. Thus, including Gibraltar, in seventeen towns, out of twenty-three, or nearly three-fourths, the greatest mortality took place in October: facts wholly incompatible with the doctrine of contagion.

In all these twenty-three towns it seems also extraordinary at first view, that the proportion of deaths amongst the females should have been much smaller, excepting in one town, Ecija, than amongst the male population. It was, in many cases one half less. In Cadiz the proportion was but 200 females, to 2692 males. Is not this fact to be accounted for, by the men being more exposed to the influence of the atmosphere? Whereas if the disease had depended upon contagion, as the women would have been more exposed, the proportions would have been reversed. In the town of Ecija, unless there be a mistake, the deaths amongst the females are nearly double, being 2,422 to 1,380. It is impossible, without local knowledge of the place, to form any satisfactory conjecture, respecting the cause of this difference; but, as an object of curiosity at least, it merits enquiry.

[1] Sir James Fellowes's Reports, table, p. 479.

In epidemic diseases, what are called relapses are universally admitted to be common occurrences [1]. Sir James Fellowes says, they "were very frequent and fatal" in the fever of Andalusia [2]. But, in a general disease, which depends upon contagion, and which is incapable of affecting the same person more than once, I am wholly unable to conceive how it is possible that a relapse should happen.

[1] Howard on Lazarettos, p. 39. [2] Reports, p. 63.

# BOOK V.

## PERNICIOUS CONSEQUENCES OF THE DOCTRINE OF CONTAGION.

## CHAP. XVIII.

*The rumour of an epidemic spreads terror to the most remote nations—important consequences of this terror—nature of the means adopted to prevent the spreading of pestilence—desertion of friends and relations—Olivier and Russel's statements respecting the opinions of the Turks—present Grand Seignor—detaining the sick in pestilential air upon compulsion—increase of mortality from concealment—terribly destructive to the poor—confessions of Dr. Mead—pestilential patients shot by the guard—murder of Simon Chiapiglia—interruption of the supply of provisions—how this would affect London, in the event of a pestilence—various other consequences of the dread of contagion.*

THE first consequence of this extraordinary delusion, respecting the cause of epidemic diseases, has been the adoption of means for their prevention, which are not only not efficient for the purposes intended, but are directly destructive to those upon whom they operate.

Under the existence of the belief in contagion, the rumour of a yellow fever in America, or the West Indies, spreads terror and alarm throughout Europe; that of a pestilence in any portion of Europe produces a similar effect in America and the West Indies; and the simultaneous appearance of an epidemic malady in several countries, creates a panic throughout the world.

The effects, both immediate and remote, of the dread, inspired by epidemic diseases, in consequence of this fatal doctrine, are extensive, various, and afflicting, almost beyond belief.

If we contemplate, in all their bearings, the various and complicated consequences of this fundamental error, whether as they relate to humanity; to the progress of medical improvement; to the moral state of communities; to political economy; to the interests of commerce and navigation; to the success of expeditions, and the safety of armaments; to individual intercourse, or to the intercourse of nations; or to the general consumer, and the public revenue; they will be found to involve considerations of an importance altogether so extraordinary, that I know of no subject of science, to which it is, in my opinion, inferior.

The means pursued have consisted in avoiding, not the air that produces the malady, but the persons that are labouring under it; and in adopting, not such measures as would insure the safety of

those, who are yet well, but such as must insure the destruction of those, who are already affected. It is an inevitable consequence of the doctrine of contagion, that the abandonment of sick friends, and relations, affords the only chance for the personal safety of those who are not yet affected; and that restriction, separation, and seclusion, can alone prevent the spreading of pestilential diseases; as well as that these constitute efficient means of prevention.

The origin of this doctrine sufficiently accounts for the difference of opinions, and of conduct, observable amongst the different descriptions of inhabitants of the Levant, so frequently the seat of pestilence: and affords a much better solution, than their speculative notions of fatalism, of the humanity of the Turks, in not deserting their friends, or relations, when they have the misfortune to be seized with the plague. "Amongst Europeans," says Olivier [1], "the tenderest ties, the closest affections, constantly yield to the alarm, which this fell malady inspires. The desire of self-preservation breaks, in a moment, through the bonds of consanguinity, and stifles the most virtuous sentiments." Under the Turkish dominion, however, each individual is left to act, according to his discretion, with respect to

[1] Travels in Egypt, &c.

the government of himself, and of his family, in times of pestilence. He is not shut up in his house, surrounded by watchmen, shunned by his neighbours, or deserted by his children, if supposed to be infected. He may seek a more salubrious air, without the danger of perishing for want of food, or being driven back into a pestilential atmosphere by neighbouring peasants, both of which happened in the plague of London, in 1665. He may even commit the crime of running abroad, in a state of delirium, without the risk of being shot by any surrounding guard[1]. That government, however, leaves the subjects of other powers, residing in their territory, at the disposal of their respective ambassadors; and they are consequently, in respect to pestilential maladies, subjected to the discipline, which has for some centuries prevailed. Very fortunately for them, it is seldom that they are in danger from the real causes of pestilence.

" Persuaded by their ideas of fatalism, that man cannot alter the decrees of the Eternal, the Turks consider it not only useless, but criminal, to take precautions, as the Europeans do, against this destructive scourge: and, if death mows them down, on all hands, they shew the utmost composure, and an entire resignation. None of

[1] A shocking instance of this is afterwards related.

them feel any repugnance to attend the sick, who are dear to them; nor can they resolve, like the Europeans, in most of the trading sea-port towns of the Levant, to abandon them to hirelings, charged too often with hastening the death of the afflicted, that they may the sooner get possession of their spoils [1]." Such, I am persuaded, is the almost universal feeling amongst the Mahommedans. There may, indeed, be some rare exceptions amongst those, who have had much intercourse with Europeans; and Dr. Russel, speaking of the plague at Aleppo, in 1760, says, "I have met with several instances, even in Turkish houses, where the mistress of the house was not only ill attended, but even abandoned, through the timidity of her daughters and slaves." The present Grand Seignor is said to be one of those, who entertains some apprehensions of this malady: and I have heard that, whilst I was in the hospital near the Seven Towers, being struck with the death of his Hunkiar-Imani, or Chaplain, he ordered the following maxim, extracted from the Hadisscherif, or collections of the sayings of Mahommed, preserved by tradition, to be brought to the recollection of the Mussulmaun people, from the principal mosques: "The sick ought not to

[1] Olivier's Travels in Egypt, &c.

have communication from without, and he, who is well, ought to avoid meddling with the sick." Whatever degree of authenticity may be attributed to these traditional sayings of the prophet (and I should be inclined to consider them very apocryphal) it is certain that no countenance can be derived from the Alcoran, to the doctrine of contagion: and, whatever private opinions, or fears, the reigning emperor may have imbibed, respecting the plague, fortunately for them, he will not be able to introduce any conformable regulations amongst his Mahommedan subjects, until some very great change shall have been affected in their general sentiments. The attempt would inevitably cost him his life. Even if the Turks could, by possibility, be prevailed upon so far to renounce their conviction of the non-contagious nature of plague, founded more, I think, upon habit and observation, than upon the tenets of their religion, as to embrace the papal doctrine of contagion, in epidemic diseases, with all its baleful consequences, their addition to the number of the believers would not prove that the belief is well-founded.

The next step in order to ensure the personal safety of those who are well, is to confine the sick, and all who are suspected of having had communication with them, to the noxious air, which is the main cause of pestilence. A more

effectual mode than this could not easily have been devised, if such had been its design, to render the healthy sick, and the diseases of the sick mortal. And to detain persons in a pestilent air, upon compulsion, which is the effect of all the usual measures of plague police, seems to me to be very little short of authorised murder.

The following are some of the most obvious, and ordinary effects of the cruel regulations, adopted with this view.

The knowledge of the treatment, which they are to meet with, in society, upon its being discovered that they are affected with the plague, induces all persons, amongst whom contagion is a creed, to conceal the disease as long as possible, lest they should be abandoned by all the world; *i. e.* until it is no longer curable: and hence a prodigious increase of the mortality.

But the effects of this dread of contagion fall with tenfold destruction upon the poor. They are not enabled to hold out, by present, or hopes of future reward, lures to meretricious attendance; and, as dread is generally more powerful than natural affection, they are not only almost certain of being left without attendance, but even without subsistence. If only suspected, they are deprived of employment: and if they have the good fortune to recover, they are still

shunned as dangerous: so that the lot of those, whom an epidemic may have spared, is often beggary or starvation.

Dr. Mead, who was in the early part of the last century, a principal agent in prolonging this delusion, and extending its baleful consequences to this country, acknowledges, that, "the methods taken by the public, on such occasions, have always had the appearance of a severe *discipline*, and even *punishment*, rather than of a *compassionate care*."

"The main import," says he[1], "of the orders issued out at these times, was, as soon as it was found that any house was infected, to keep it shut up, with *a large red cross*, and '*Lord have mercy upon us*' on the door; and watchmen attending day and night to prevent any one going in, or out, except physicians, surgeons, apothecaries, nurses, searchers, &c. allowed by authority: and this to continue at least a month after all the family was *dead*, or *recovered*[2]."

"It is not easy to conceive a more dismal scene of misery than this. Families seized with a distemper, which the most of any requires help and comfort, lockt up from all their acquaintance;

[1] Mead's Discourse, p. 32.

[2] Directions for the cure of the plague, by the College of Physicians; and Orders by the Lord Mayor and Aldermen of London, published 1665.

left it may be to the treatment of an inhuman nurse (for such are often found at these times about the sick,) and strangers to every thing but the melancholy sight of the progress death makes among themselves; with small hopes of life, and those mixed with anxiety and doubt, whether it be not better to die, than to survive the loss of their best friends, and nearest relations [1]."

What seems most unaccountable in this matter, is, that physicians, surgeons, apothecaries, nurses, and searchers, &c. should be supposed less capable of transmitting the disease, if it were contagious, than the friends, or relations, who might visit the sick.

The operation of this belief is frequently attended with the most distressing occurrences:

"I was informed," says Howard, "that, lately, in a hamlet belonging to the Ragusian state, all the inhabitants died of the plague, *thus imported*, with the exception of two or three, who were themselves *shot, by order of the magistrates to the surrounding guard* [2]."

"The 30th of January, 1784, it was perceived that a man, called Simon Chiapiglia, from the burgh of Luzaz, belonging to Spolata, after five days of fever, had a tumor in the arm-pit, of so much the more *suspicious* a nature that he

[1] Mead's Discourse, p. 32, 33.

[2] Lazarettos, p. 22.

had been employed as a door-keeper, in a lazaretto, from which he had been discharged on the 21st of the same month, after having been made to perform quarantine. He was well watched: but on the following day, as, in a fit of delirium, he endeavoured to escape, he was *killed with a musket shot* by the centinel. No other suspicious spot appeared upon his body. His family were sent to the lazaretto, but they remained in good health[1]."

During the plague in London, in 1665, every one was afraid, in the country, that it would be communicated to them by travellers. The consternation was beyond description. No one, but under the most pressing necessity, would sleep in a tavern.

One of the most destructive effects of terror, from the belief in contagion, is the interruption of the supply of provisions, in times of pestilence. This was dreadfully experienced both in the plague of London, in 1665, and in that of Marseilles, in 1720. Whenever an epidemic exists in the Barbary states, the intercourse between them, and our colonies of Malta, and Gibraltar, is suspended; and a principal source of supply is immediately cut off. We shall perhaps have a more distinct idea of the operation

[1] Lazarettos, p. 46.

of this belief, if we contemplate what would be its probable effects, in the event of a pestilence in London, in the actual state of its population.

A pestilential atmosphere depending upon changes, which we are not yet enabled precisely to calculate, the periods of its recurrence are of course wholly uncertain; *i e.* to us wholly unknown. There does not appear to be any foundation whatever, for the opinion entertained by Sydenham, and other writers, that epidemic diseases recur, at regular intervals, of a certain number of years. That they occur in some countries less frequently, and in others more frequently, than formerly, is a fact that does not however seem liable to question: and this, I think, is in proportion, other things being equal, to their respective advancement, or retrogradation, in cultivation, civilization, and knowledge. In the Turkish dominions, for instance, which have considerably retrograded, in those respects, the plague is more frequent in its appearance, than formerly; or, is seldom absent for a whole year; whilst, in England and France, which have advanced in more than an equal degree, it is much more rare, and has not for a long time occurred; a preference, by the bye, which diseases depending upon contagion, never shew.

But, however the frequency, and mortality, of epidemic diseases diminish, cæteris paribus, with the progress of improvement, it is certain, that, neither civilization, cultivation, or knowledge, can prevent the occurrence of a pestilential atmosphere; and consequently, that no nation, however advanced in these respects, can promise to itself a permanent exemption from plague.

Every individual on the face of the earth, is therefore, at all times, deeply, although they are not all equally, interested, in the subject of the present enquiry. And this interest, on an average of all the nations of the world, has, perhaps, increased, rather than diminished, in modern times. For, if, in the countries most advanced in civilization, some of the minor circumstances, which tend to aggravate epidemic diseases, may have wholly disappeared, and others considerably diminished in force; yet, there is no doubt that we continue as liable as ever to the recurrence of the grand cause of those maladies—a noxious state of the atmosphere: and, although this cause, being deprived of the aid of those concurrent circumstances, which depend upon the backward state of society, does not produce the same destructive consequences as formerly; still a great, although a less perceptible mortality, must frequently be produced, by the noxious

qualities of the atmosphere alone, aided by the circumstances attending an increased population, and perhaps by the increased difficulty, amongst the poorer classes of that population, of procuring the means of subsistence.

If we examine the bills of mortality, in London, we shall find, in some months, or weeks, double the number of deaths, that happen, in the same months or weeks of other years; and, although the symptoms are not sufficiently marked, to constitute a declared pestilence; there is no question, that, the excess of deaths depend upon the state of the atmosphere, and that there has prevailed an obscure epidemic, or mitigated plague. Thus, in the week ending the 15th of December, 1812, there died 653, which is more than double the number of deaths, in the week ending the 17th of December, 1811, being only 310.

But 653, the bill ending the 15th of December, 1812[1], equals the whole number of deaths, at various periods, during the plagues, which occurred between 1592 and 1665, when the mortality, from plague, was put down, in the bills, at 3, 4, and 500. And as, under the ordinary circumstances of the atmosphere,

[1] I did not select this bill from amongst a great many, but merely from those of a few weeks, of a few years, which happened to be put into my hands. The subject is worthy of, and shall have, further consideration.

the average deaths at present, the population being doubled, do not much exceed the average deaths at that period, the number that would then have been so regarded, may now equally be considered to amount to an epidemic. But, as, until the symptoms of the higher degrees of pestilence appear, the existence of such a malady is not suspected, the adventitious causes, or the consequences of the belief in contagion do not operate. If, when the weekly bills were at 653, any unequivocal symptoms of plague had, in a few instances, been discovered, the alarm would have immediately spread; terror would have begun to operate; and, according to my estimate of its usual effects, mortality would have been multiplied fourfold. It is the alarm alone, then, which would have occasioned so vast a difference. And I question much, whether if a declared pestilence were to arise, the increased difficulty, under the operation of the belief in contagion, of supplying the increased population with subsistence, would not more than counterbalance the advantages derived from the improved condition of society: and whether the mortality under a similar state of the atmosphere would not be at least in the precise proportion of the population.

Now, let us enquire what effects such an atmosphere, as occurred in 1665, would proba-

bly produce, in London, with its present population, and under its present circumstances. We shall suppose, that, in the improved condition of the city, an air of the same degree of insalubrity would only produce half the number of deaths which took place in 1665, in proportion to the population. This would certainly be the case. But, although we allow, that, as far as the air, and the circumstances of the city, combined, are concerned, this would be the proportion; yet, as, under the belief in contagion, fear, and all the other consequences of that belief, must be presumed to operate as before, the number of additional deaths, depending upon these causes, would be in the precise degree of the increased population.

Of the mortality, incidental to pestilential diseases, I have supposed three-fourths to be occasioned directly by the consequences of this belief; or what I have called the adventitious causes of mortality. It is stated, upon apparently good grounds, that the number of deaths from plague, in 1665, must have exceeded 100,000, although the bills of mortality gave only 68,596[1]. One fourth of this number, or 25,000, I estimate as the proportion that were victims to the intrinsic

[1] History of the Plague by H. F. The Plague at Marseilles, considered by Richard Bradley, F.R.S. p. 19. Tables of mortality in the Appendix.

severity of the disease, or to the effects of the proper causes, and 75,000, as the proportion that were victims to the effects of the adventitious causes, or to the consequences of the belief in contagion [1].

The population of London being now double, whilst the mortality, under the ordinary states of the air, remains nearly the same, the deaths from the proper causes, under a similar state of the atmosphere, with that which occurred in 1665, would still little exceed twenty-five thousand; but that from the adventitious causes, increasing in the precise proportion of the population, would be doubled, amounting to a hundred and fifty thousand. Thus, in an air similar to that of 1665, the deaths from both causes, the belief in contagion remaining the same, would amount to one hundred and seventy-five thousand; whereas, that belief being renounced, and all its consequences having ceased to operate, the whole number of deaths, under a similar state of atmosphere, would not much exceed twenty-five thousand, or one seventh part. According to this computation, the simple renunciation of error, without including an efficient method of cure, would obviate six sevenths of the otherwise inevitable mortality.

[1] See note 1, at page 444.

Although there can be no doubt, that, from the improved condition of societies, in modern times, a more highly noxious state of the atmosphere is necessary to produce the same degree of disease, and that plagues will consequently continue to be much less frequent than formerly, in civilized countries; yet it cannot reasonably be inferred, either that they will always remain exempt from them, or, that, when they do occur, their mortality will not be as great as formerly.

For entertaining such opinions, the following are my reasons. Although it be true, that there has been no palpable pestilence in this country, for 150 years, we do not find any reason, in the nature of things, to justify the conclusion, that this exemption will be perpetual. And it is most certain, that, if a pestilence were to happen in London, the belief in contagion continuing as strong, and as general, as it is at this moment, the mortality would be proportionally as great as formerly, if not greater. Any one, who peruses the history of the plague of London, in 1665, will perceive the difficulty, which existed, of provisioning the city, owing to the existence of that belief. How much more, then, would that difficulty be increased now, when the population is doubled? It would, I am persuaded, be impossible, *under the full operation*

*of that belief, and with all the measures of precautionary police, necessarily resulting from it, in activity,* to provide for the regular supply of the present population of the metropolis, to any reasonable degree; and, if the inhabitants should also be prevented from going into the country, as was then the case, those would fall victims to famine, who might escape disease. Upon that occasion thousands perished, from want, in the fields[1].

But should we, in this country, contrary to the probable course of events, continue permanently exempt from the higher degrees of pestilence, (to the lower ones we must ever remain subject,) the nation would still possess that interest in the investigation, relating to this class of diseases, which it does, in the alleviation of every other species of misery, amongst our distant fellow-creatures, arising from the common sympathies of humanity. Nay, if these sympathies could possibly be extinguished, we should still have a strong direct interest in the enquiry, arising from the frequent occurrence of pestilential diseases, in the British possessions of Gibraltar, Malta, and the Ionian islands, in the colonies in the West Indies; and even in many parts of our extensive East Indian territory.

[1] History of the Plague, by H. F.

If the mortality, incidental to epidemic diseases, has decreased in civilized countries, it has increased, in perhaps a greater proportion, according to their increase in population, in those which are stationary in improvement, and farther still in those, which, like the Turkish dominions, are in a state of retrogradation. Consequently, as involving objects, which concern the welfare of all nations, at all times, it is evident that a discussion of this nature cannot but possess an universal interest; and that as it has already been too long delayed, it cannot be too early entered upon.

The dread of contagion prevents medical men from giving attendance to pestiferous patients, or exposes them, if they do, to quarantine, lest they should infect their neighbours. Dr. Hodges, we are told, prescribed from a parlour-window, to patients in the streets of London. Dr. P. Russell prescribed from a chamber-window, fifteen feet above the level of the streets, at Aleppo. The physicians at Oxford, fled from dread of the malady, which arose at what was called the black assize in that town. The Russian physicians, during the plague of Moscow, in 1771, wore cloaks dipped in vinegar, and hose plastered with pitch. During the yellow fever, in Philadelphia, in 1793, many of the physicians abandoned the city. In the Levant, it is seldom

that pestiferous patients have medical aid. The physicians who venture to visit them, wear oil-skin dresses, and use other means, much better calculated to create additional terror in their patients, than to preserve themselves.

By Statute I. Jacob. I. c. 31. *Infected* persons going abroad after command to keep at home, may be resisted by watchmen, and punished as vagrants, if they have no sores on them: if they have *infectious* sores, it is *felony*[1].

In Zante, the peasants returning from the Morea, in harvest, are obliged to perform a seven days quarantine. Other persons, coming from the Morea, *when there is no plague*, are made to perform a quarantine of fourteen days[2].

It seems it used to be the custom, and is in some places still practised, to drive away, or burn vessels having the plague on board. "Ships which have the plague on board are now received here (at Leghorn), and not chaced away, or burnt, *as is practised in too many places*[3]."

In corroboration of this practice is the following statement: "In one of the enclosures (at Venice) was the crew of a Ragusian ship, which had arrived a few days before me, *after being driven away from Ancona and Trieste*[4]."

[1] City Rem. p. 333, note. [2] Howard on Laz. p. 9.
[3] Ibid. p. 7. [4] Ibid. p. 11.

At Malta, in 1813, during the height of the epidemic, at the rate of ninety in the hundred, or nine in ten, of the sick, died; and, of between eighty and ninety, that were sent to the lazaretto hospital, ONLY TWO SURVIVED. Had these men, in the lazaretto hospital, any medical treatment? Did an individual of the medical faculty visit them? Or, if so, did he do more than merely to look at them, from afar? Who, indeed, knows, for what person was present in the Hospital, who could be an impartial evidence, that, the men so situated, had any assistance, of any kind?

This year (1813) the garrison of Gibraltar, as we learn, "was in strict quarantine, *for several months*, before the malady made its appearance; and a board of health was sitting, almost daily, on account of the plague, which had broken out at Malta[1]."

The moment the kind of atmosphere, which was calculated to produce it, had reached Gibraltar, the epidemic made its appearance; afterwards run its course; and terminated, without any reference to the presumed operation of contagion, marsh-miasma, quarantines, lazarettos, or other institutions of plague police. And it is very certain that the effect of these institutions, in as far as they operated, must have

[1] Burnet, p. 479.

been in many ways pernicious to health and life, but especially by confining people to the air which was the main cause of the malady.

Thus, although the intrinsic severity of a disease be precisely the same, whatever may be its proper cause, it is obvious that in epidemic diseases, both sickness and mortality must have been enormously encreased by all the adventitious causes of disease, misery, and death, which, have for the last 270 years, flowed from the doctrine of contagion : and when I say that the increase must have been fourfold, I do not imagine that I exaggerate [1].

Since to produce disease must be the obvious and necessary consequence of detention and confinement, under circumstances calculated to depress the mind, or to deprive the body of its usual exercise, it cannot be necessary to cite proofs of the insalubrity of quarantine, and

[1] The causes of mortality, in epidemic diseases, may, for convenience, be distinguished into physical, and moral, or proper, and adventitious. Upon the physical, or proper causes, *i. e.* the properties of the atmosphere, &c. depends what may be called the intrinsic severity of the disease, which is here supposed to produce only one-fourth of the whole mortality ; the remaining three-fourths being presumed to be occasioned by the moral, or adventitious causes, *i. e.* the consequences of the erroneous belief in contagion.

other institutions of plague-police. They are to be found in the most ordinary laws of life. They are particularly exemplified in jail, hospital, and ship fevers. Howard complained of constantly experiencing head-ach during his visits to the lazarettos and hospitals [1].

Until this pernicious delusion shall be renounced, although individuals may possibly contrive, at considerable personal risk, to make some useful experiments, the obstacles, to a full, free, and efficient, investigation in pestilential diseases, will always remain insurmountable. The sick will continue to be isolated, and sacrificed; persons, in health, to be frightened into sickness, and abandoned; and physicians to be deterred, by their participation in the prevailing error, and consequent dread; by the fear of being obliged to perform quarantine; and by the apprehension of losing their ordinary practice; or by all of these circumstances combined, from giving, even the smallest attendance to the poor pestiferous patient.

Under the prevalence of this most extraordinary belief, from the combined effects of the operation of dread, upon already enfeebled minds; of the alarm inspired, or perhaps the want occasioned, by the desertion of friends, relations, and attendants; and of the diminished

[1] Lazarettos, p. 106.

confidence, and absolute privation of physical support, depending upon the destitution of medical aid; from all these circumstances, added to the intrinsic severity of the disease, the unfortunate being, who happens to be seized with a pestilential malady, has but a very slender chance of escaping with life.

Considered in regard to the progress of medical improvement, besides that the very existence of this delusion is an opprobrium medicorum of no common degree, its operation forms a permanent obstacle to improvement, and an insuperable bar to the application of curative means, in the most numerous, and most dangerous class of maladies, which infest mankind. Such a belief equally precludes investigation, and the employment of known remedies.

The consequences of this error, cannot but add to the usual demoralizing influence of such horrid scenes as are presented, in all severe pestilences, in the precise proportion of the additional aggravation; obliterating the ordinary feelings of humanity, destroying the sense of propriety and decorum, and, in extreme cases, even producing a suspension of social order [1].

[1] The histories of the plagues of Athens, of London, and of Marseilles, abound with instances in point.

## CHAP. XIX.

*If epidemic diseases were contagious, quarantine should be universally established.—Plan of Count Harrach to that effect, presented to the Congress at Vienna.—But as they do not depend upon contagion, their universal adoption would be universal ruin.—Consequences of their establishment in Turkey contemplated.—Proposed by the Court of Vienna to the Ottoman Porte—but declined—Of Quarantine, Lazarettos, Bills of Health, and Plague Police, as they now exist.—Their pernicious effects upon commerce and navigation; the intercourse of individuals and of nations; naval, and military expeditions; the general consumer; and the public revenue.—If infection existed in goods, quarantine could be of no avail.—Institutions of Plague police ought to be either universally extended, or universally abolished.*

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Were it possible, by the general consent of nations, to establish an equal, and just, code of regulations; and could these regulations be rendered an efficient barrier, against the introduction, and spreading, of epidemic diseases; it would undoubtedly be proper, in consideration of the great benefits, which would, in that case, be derived from them, to submit patiently, and even chearfully, to the inconveniences, which they would incidentally produce.

In that case, the measure proposed by the philanthropic German physician, Count Harrach, to the Congress of Vienna, would have been highly expedient; and it would have been the duty of all governments to have united in enforcing the universal adoption of his plan. It was liberally communicated to me by the benevolent author, at the end of 1815, knowing me to entertain opinions upon the subject, of a diametrically opposite nature; and is as follows:—

MEMOIRE.—L'Epoque actuelle ou la Providence en dirigéant une réunion d'efforts les plus nobles et les plus énérgiques, a rendu la paix à l'Univers, est un moment à jamais memorable, que la posterité la plus reculée ne cessera de bénir.

Une grande partie des Souverains les plus genereux et les plus humains, leurs ministres et les réprésentants de tous les états civilisés, excepté l'Amerique, sont reunis ici pour consolider ce grand et salutaire ouvrage.

Rien de ce qui peut contribuer au bonheur des hommes ou en diminuer les suffrances, ne sauroit être étranger à cette auguste réunion; au contraire, tout ami de l'humanité, qui se sent penétré de cette sublime vocation, doit être sûr, que toute proposition tendante à ce noble but sera acceuillie avec bienveillance, et soutenue au Congrés.

C'est dans cette douce attente que l'auteur ose soumettre à la sagesse du Congrés les idees suivantes.

Parmi les maux qui affligent les hommes réunis en Societé la Peste du Lévant est sans contredit un de plus terribles : malheureusement cette verité n'a pas besoin de commentaire. Ce n'est pas seulement comme mal physique, que l'idee de la peste fait frêmir, mais encore comme mal moral, parceque, excepté les individus, qui se vouent et se sacrifient héroiquement pour leurs semblables ; ce fléau demoralise ordinairement le reste.

La nature de la Peste jusques à présent inconnue comme cette de tant de maux, que l'Esprit humain n'a pas encore penétré, est du nombre de ceux que l'on n'a pû combattre avec succés ; que dans quelques cas isolées, mais dont l'effet horrible ne peut être évité qu'en écartant avec des soins assidus le fléau même.

La seule garantie contre cette calamité est une Quarantaine bien organisée, et strictement observée. La ci-devant Republique de Venise, la Russie, mais surtout la Monarchie Autrichienne, ont été plus d'une fois les gardiens de l'Europe, et l'ont sauvé par la de la contagion generale. Encore dernierement les mesures prises en Transilvanie à l'occasion de la Peste de Cronstadt, ont confirmé la verité de cette assertion.

Mais que font les meilleurs etablissements de Quarantaine, qui n'ont lieu que du coté de la Puissance, sur le territoire de laquelle le mal peut être transféré, si en même temps il ne s'en trouve pas d'établis, ches la Puissance, dans les états de laquelle le germe encore indestructible de ce fléau à pris naissance et ne cesse de se developper.

Le voeu de l'Auteur de cet écrit, et de tous les vrais amis de l'humanité est donc que les Souverains et leurs ministres engagent par des réprésentations unanimes et énergiques la sublime Porte, à organiser dans ses états la Quarantaine, à l'instar de celles déja existantes, avec lesquelles il sera tres facile de les mettre à l'unisson.

Les puissances chretiennes de leur coté préteroient naturellement la main à l'exécution du projét. Il y auroit d'autant plus lieu de se flatter du succés, que dans ce moment ce serai la voix et le voeu de l'Europe réunie qui se feroient entendre, et qu'il existent deja des exemples, que la Quarantaine n'est pas si absolument contraire et étrangère aux Musulmanns qu'on a lieu de la craindre.

Dût ce projèt être réalisé en tout ou en partie, ce sera un beau Triomphe de l'humanité, digne de l'Auguste Réunion à laquelle il est soumis.

De tous le projèts présentés au Congrès celuiéi est probablement le seul qui ne se trouve en collision avec aucun autre tant sa source est pure,

et son objet sacré. Aussi son Auteur se trouve-t-il recompensé d'avance par la douce confiance, avec laquelle il a osé en appeller aux sentimens les plus genereux et les plus humains des Souverains et de leurs ministres.

HARRACH, Docteur en Medecine.

But, far from being efficient barriers, against the introduction, and spreading of epidemic diseases, we find that the effect of these regulations, is, to aggravate, in a dreadful degree, the otherwise inevitable calamities of pestilence.

These institutions were necessary consequences of the belief in contagion, as the cause of epidemic diseases. The governments bordering upon the Turkish dominions, regarded as the most ordinary seat of pestilence, either partaking themselves of the panic, which its vicinity never fails to create, or constrained to act according to the opinions or prejudices of their subjects, were necessarily impelled to take measures of precaution, founded upon the prevailing notions respecting its cause. Nor could they, these notions remaining the same, have hastily ventured to abolish the system of seclusion, separation, and restriction, thus adopted, even had its inefficacy been demonstrated. Hence the continuance, of quarantine, lazarettos, and other police institutions, for preventing the introduction and spreading of pestilential diseases,

in the Southern and Catholic nations of Europe. Amongst these, the state of Venice took the lead.

In those of the North, the same measures of precaution have not been so generally adopted, owing in part to their greater distance from the ordinary seat of the malady, and the consequent absence of that terror, which its vicinity inspires; and, in part also perhaps to the less impression originally made upon their minds, by the delusion, from their not placing, such of them at least as have become Protestant, equal confidence in the infallibility of the Pope.

The government of Holland, actuated by a spirit of commercial calculation, or perhaps by a more enlightened view of the subject, has uniformly refused to receive the pernicious regulations in question.

It was not until some time after the plague of Marseilles, in 1720, that they obtained a footing in England. Every one must feel both surprise and regret, that the government of an enlightened country, meditating coolly, and at a distance from the ordinary seat of the evil, without the plea of immediate dread, or popular prejudice, strongly impelling them, and with the evidence besides of nearly three centuries of their more than doubtful effects in other countries, should have been induced, to adopt regulations, which, if not adequate to their professed object, could

not but be a source of gratuitous inconvenience, injury, and expence. The decisions of the Privy Council, upon this occasion, appear to have been chiefly founded, at first, upon the opinions of Dr. Mead, and, afterwards, in the transfer of quarantine, from the Mediterranean stations, to England, by the notions of Mr. Howard, and the Levant merchants, that our Levant commerce would be thereby, placed more upon a par with that of the Dutch. It might have been expected that the advanced state of general knowledge, and the extreme importance of the subject, should have suggested to the Privy Council the danger of allowing themselves to be guided, in such weighty decisions, by the flimsy materials then within their reach, as well as the expediency of instituting an investigation, upon an extensive and permanent basis, into the nature and cause of epidemic diseases, before determining on any final arrangement, respecting the means of their prevention.

It seems, however, to have been thought sufficient, and perhaps it was so in a mere view of responsibility, to have adopted, and acted upon the new traditions of the Levant, and of Italy, which, with the deceitful semblance of presumed experience, have left every part of the subject involved in endless perplexity, and almost inextricable confusion.

In the early ages, when it was imagined that

plagues were sent directly from Heaven, to punish men for their sins, the means resorted to, for prevention, naturally consisted in prayers, sacrifices, and lustrations: and, even after these attempts, to appease the supposed anger of the gods, were found, by fatal experience, to be unavailing, the multitude still persevered, to the neglect of such rational means of prevention as were immediately in their own power, in the same fruitless endeavours.

These superstitions of the ruder ages, were, however, as to the object in view, only simply inefficient. They were not, in any manner, mischievous. Whilst the measures of prevention, adopted by the governments, and sanctioned by the legislatures, of these modern and comparatively enlightened times, are not only, inefficient, as to the object in view, but, independently of the burthen of large and expensive establishments, in a variety of ways, extensively injurious to mankind.

They are, in effect, a real grievance perpetually operating, with a view to obviate an ideal danger. In this country, they have cost the nation, for the last fifteen years, independently of the detention of ships, crews, passengers, and merchandise, at least £300,000: and it signifies little whether this sum be taken, in the first instance, from the public revenue, or the Levant Company; it all

comes ultimately from the pockets of the general consumer.

But now, that the fallacy upon which they were founded, has been detected, and exposed; and that the pernicious operation of quarantine, lines of circumvallation, ditches and cordons of troops, round cities *supposed to be infected*[1], as well as of every other police regulation, for enforcing separation, seclusion, and restriction, has been demonstrated; surely it would be unpardonable to suffer one moment to elapse, without taking measures to ascertain the expediency of their immediate and total abolition.

In consistency, it must be contended, by the advocates of contagion, that, if quarantine was abolished at Marseilles, Malta, Genoa, Leghorn, Trieste, Gibraltar, and the Ports of Spain, the plague would appear, as often as it occurred in any of the sea-port towns of Turkey, with which these places are in commercial intercourse. Such would certainly be the necessary consequence of

[1] "*Simply* suspected," "*grievously* suspected," and "*most* suspected," are the terms of comparison, employed, in such cases, by the craft and mystery-men of the Lazarettos, in the *Levant*. *Howard on Lazarettos, p.* 47. This language reminds me of the "*suspectées d'être suspectes*" of the ferocious periods of the French revolution: and in both cases, the words were probably of equally ominous import, to the unfortunate persons, or cities, whom they designated.

the truth of the doctrine of contagion. But, it is well known, not only, that no such effects arise, even in appearance, from commercial intercourse; but, that, in Gibraltar, and the towns of Spain, the recurrence of pestilence has been more frequent, since the introduction of plague police regulations, and especially since they have been brought to the most rigid state, than at periods anterior to their existence. In 1665, the plague, in London, spread most rapidly, and proved most fatal, at the very period, that the shutting up of houses, *supposed to be infected,* and other restrictions upon intercourse, were most rigorously enforced: and it was after the houses were again laid open, and the people had abandoned all other precautions, in despair, that the spreading of the disease, and its mortality, suddenly diminished, and ceased[1]!

Can any thing in nature be more conclusive? But it is also obvious, that, as these institutions are not only inefficient for their pretended object, —that object indeed having no existence, but also more positively pernicious in a period of actual plague, than in a period of exemption, their injurious consequences, in any country, must increase in the precise proportion of the frequency, ex-

[1] History of the Plague by H. F. The shutting up of houses soberly debated, 1665. Mead's Discourse, p. 37.

tent, and severity, of the recurrence of pestilential maladies. In countries, in which they appear frequently, they would be intolerable. To illustrate this position more forcibly, let us suppose their general establishment in Turkey, where the plague almost always exists. In how dreadful a situation would that empire instantaneously be placed! One half of the inhabitants of the seaport towns would be shut up in quarantines and Lazarettos, more than three-fourths of the year, and the other half employed to provision and guard them. Commerce, navigation, and all human intercourse, would be interdicted. Even the remote husbandman, although he might reap his harvest, durst not bring it to market. Motives to labour would diminish. Terror and despondence would increase. Famine would destroy whom the disease might spare. And the population of a great country would speedily dwindle into the insignificance of wandering hordes.

This is no ideal picture of what would be the condition of the Turkish empire, in almost every town and province of which the plague frequently, in some almost annually, and in several almost constantly prevails, were the police establishments of their European neighbours, for preventing the introduction and spreading of that disease, introduced amongst them. Yet such was the measure seriously proposed, by the Austrian

government, to that of Turkey, in the reign of Selim IV. with the view of extirpating epidemic diseases. That good, but in some respects weak Prince, desirous of adopting every innovation, which he considered an improvement, however impracticable, had himself no disinclination to entertain this proposition of the Austrian cabinet, which was formally presented to the Porte, by the present worthy Internuncio, Baron Sturmer [1]. But it was necessary that it should be first submitted to the Divan. On this occasion, the Mufti spoke with great vehemence against an innovation, which he considered unjustifiable: and the design was abandoned.

In a pamphlet, entitled "Suggestions for the Prevention and Mitigation of Epidemic and Pestilential Diseases," &c. recently published, I endeavoured to shew, that, even according to the doctrines of the advocates of contagion, quarantine, Lazarettos, and plague police establishments, are at least useless. Having here demonstrated that the object, for which they were instituted, does not exist, it follows, *a fortiori*, that they can be no avail. It now, therefore, only remains to prove, that they are positively and greatly pernicious.

[1] I was favoured with these facts by Professor Frank, of Vienna, who was himself a member of a committee appointed to frame a set of quarantine regulations, intended to be proposed for general adoption.

Whether the measures which have been pursued, in order to prevent the propagation of supposed infection, consist of seclusion and confinement, in a house, lazaretto, ship, district, or town, they are all of the same nature, differing only in degree and manner of restraint. The shutting up of houses, in which there are persons ill of the plague; drawing lines of circumvallation, and cordons of troops round cities, supposed to be infected; shooting the sick; and other measures of similar barbarity, of which there are examples but too recent, are, I trust, already beginning to grow obsolete. And let us also hope, that it will be but a very short time before we see the detention of ships, goods, and persons, upon grounds wholly chimerical, proscribed, as unsuitable to the spirit and intelligence of the age.

All the regulations of plague police may for convenience be represented by the general term Quarantine.

Quarantine signifies the detention for forty days, as the term imports, in a state of exclusion from society, of persons, ships, goods, &c. supposed to be capable of propagating, by contact or contiguity, an infectious disease; forty days being presumed to be the period at which that capability ceases.

Although the periods vary, being sometimes

more, and sometimes less than forty days, the appellation remains the same.

As quarantine is a measure in perpetual operation, whether there exist an epidemic disease or not, and even without the rumour of one, it becomes a consideration of more importance, in so far as its effects are distinct from those of other plague police measures, than the regulations which are only enforced in the event of actual pestilence.

Were the laws of quarantine in other respects well-founded, we might justly accuse them of having had more regard to the fears, than to the permanent interests of persons in health; whilst to the sick they have been cruel, pernicious, or destructive. Plague, in particular, has been assumed to be a disease necessarily fatal; and the measures which have been adopted upon its occurrence, have been well calculated to justify the assumption. In resolving upon means proper to arrest the progress of an evil, of which both the nature and the cause have been equally misunderstood, it appears as if the suggestions of alarm had been alone consulted. These regulations, one would think, must have been made by men in high health, and never expecting that it might come to their turn to be afflicted with sickness; for they are the quintessence of abject fear and base sel-

fishnesss. By the very precautions employed against their spreading, we find diseases, which might only be severe, rendered almost certainly mortal; and innocent or useful members, for having the misfortune to be seized with a dangerous malady, lopped off, like criminals, from the great body of society [1].

As Quarantine regulations will, in future times, be regarded as a subject of curiosity, or perhaps their existence discredited, I shall here state what they actually are in Great Britain.

### DIFFERENT KINDS OF QUARANTINE.

Ships, goods, and persons, are declared liable to quarantine "coming from, or having touched at, any place from whence his Majesty, with the advice of his Privy Council, shall have adjudged and declared it probable, that the plague, or any other *infectious* disease or distemper, highly dangerous to the health of his Majesty's subjects, *may* be brought; and those having communication with, or receiving any persons or articles from ships

[1] It would not be practicable here, without extending these remarks much beyond their intended limits, to enter into a distinct analysis of the effects of each separate measure of plague police. But those, to whom these observations are particularly addressed, will be able to distinguish to which measure they are more especially applicable, when they do not embrace several, or the whole.

so coming from, or having touched at, such infected place," &c [1].

" The places from whence the plague, or some other *infectious* disease, may be brought into Great Britain, &c. are adjudged to be any part of Turkey, or any port or place in Africa within the Straits of Gibraltar and the West Barbary, on the Atlantic Ocean [2]."

But we find, that " although certain goods and ships, under particular circumstances, are made subject to certain fixed rates of quarantine, yet it is farther provided, that goods specified in *any* Order of Council, and ships, shall be made subject to quarantine [3]. The Privy Council may make such orders as they shall think necessary upon emergencies [4]."

Thus it appears, that ships and goods coming from any part of the world, and under any circumstances, may be subjected to such restraints and detention as the Privy Council may choose to direct: " they may make such order as they shall think necessary upon emergencies."

[1] 45 Geo. III. c. 10. s. x.; and O. C. 5th April, 1815, s. i.

[2] Order in Council, 5th Ap. 1805.

[3] 45 Geo. III. c. 10. s. xi.

[4] Ib. s. vii.

This unlimited authority has of course given rise, as must always necessarily happen in such cases, to many injurious acts and regulations. If, for instance, a vessel meets with another vessel, or a squadron, at sea, and is boarded by them, although those so communicating have neither sickness, nor have come from any place at which sickness existed at the time of their departure, she is obliged to do quarantine. I knew a case of this kind, in which a ship (the Sophia and Mary, of Bristol) was obliged to perform a quarantine of forty-two days at Malta, merely for having been boarded by an Algerine squadron off Lisbon.

The bare rumour of the existence of a plague in any part of the Mediterranean, gives a pretext for the strict enforcement of quarantine amongst the farthest nations of the North ; and the report of a yellow fever in the West Indies, produces an increased vigilance of the plague police establishments on the shores of the Baltic. Of the injury which these have occasioned to commerce and navigation, in this country, some idea may be formed, from the following sweeping clause, which it has been latterly judged expedient to introduce, in order to protect our Levant trade against the competition of the Dutch, and others, who were enabled, by the lenity of their quarantine laws, or an almost total disregard of them, to anticipate, and undersell us, with respect to

Turkish commodities, in the British markets: "Certain goods, wares, and merchandises, being the growth, produce, or manufacture of Turkey, or of any place in Africa, within the Straits of Gibraltar, or in the West Barbary, on the Atlantic Ocean, coming from any place in Europe, without the Straits, or on the Continent of America, where there is not a regular establishment for the performance of quarantine," are declared subject to the same regulations as if coming directly from the Mediterranean, or the West Barbary, on the Atlantic Ocean [1]. The ships which bring them shall do quarantine, like those from Turkey, with clean bills of health [2].

It can hardly be supposed, that to guard against contagion could have been the design of this regulation; since, with that view, it would have been perfectly ridiculous. And although, with what I understand to be its avowed object of protecting our commerce against the rivalry of the Dutch, it may appear somewhat less irrational; it would certainly have been still better, that such a measure had not been rendered necessary. The more numerous, complex, and rigid, are our quarantine regulations, the more will our positive, as well as our relative advantages, with regard to the Levant trade, be diminished.

[1] O. C. 5th Ap. 1805.

[2] O. C. 5th Ap. 1805, s. v. and xlii.

There are, in this country, six kinds of quarantine.

1. That performed, on account of actual pestilence, or *suspected* sickness, at the isles of Scilly only, its period being forty-four days.

A ship bound from Turkey, we shall suppose, arrives in England. She has one, or a few sick persons on board. The disease is immediately concluded to be contagious, or *suspected*, which has the same effect; although it would be difficult to conceive how one or two sick persons, having an infectious disease capable of being propagated, as is supposed of plague, could make a long voyage, in daily or hourly contact with their messmates, without communicating the malady to all the crew. But there is probably no instance in the annals of the trade between Turkey and this country, of the arrival of a vessel having, or having had, her whole crew sick. Now it is hardly possible to suppose, that any one can have about him that potent and penetrating kind of contagion, imputed to the plague, without his communicating it to the whole crew; although a disease may certainly affect a whole crew, scurvy for instance, without being in the least contagious.

These trifling inconsistencies, however, being disregarded, quarantine ensues. In the mode

of performing it, one should suppose that some regard might be had to the greatest infectious distance (five paces) laid down by the lazaretto authorities of the Levant. But no! neither five, nor five hundred, nor yet five thousand paces, are deemed sufficient by the plague authorities, in this country, to ensure safety. They do not consider themselves as secure from infection, until this unfortunate ship and cargo, her crew and passengers, sick and well, articles enumerated, and articles not enumerated, as contagious, have fairly reached the rocks of Scilly [1]!

It is farther enacted, that if any death, suspected to arise from a contagious disease, happen during any period of the quarantine, it shall recommence [2]. And thus, if a suspected death should take place on the last day, the ship, crew, passengers, and cargo, must submit to a farther detention of forty, or forty-four days; and it might even so happen that, from an unfortunate repetition of such an occurrence, the detention might be prolonged until the ship and

[1] "Commanders, or masters of ships, having an actual pestilence on board, are directed, if within the Straits, to proceed to some lazaretto in the Mediterranean; if without the Straits, to proceed to the Scilly Isles." 45 Geo. III. c. 10. s. xiii.

[2] O. C. 5th. Ap. 1805, s. xxix.

cargo were rotten, or her crew and passengers dead [1].

Such a chain of accidents, it is true, may not often happen; but the law is not, on that account, the less injudicious. If the evil were of frequent occurrence, its immensity would alone demonstrate the absurdity of the enactment, even to those who did not immediately feel the injury.

But it is so seldom, if ever, that quarantine is performed in England, on account of *suspected* sickness, that, but for the principle, the point would scarcely have been worthy of any, and,

[1] This would certainly be their fate, under the circumstances stated, if plague, being capable of affecting the same person repeatedly, were really contagious. All these regulations have been literally copied from those of Venice, without considering that, even had their foundation been correct, the difference of the circumstances of the two countries would have required an essential difference in the application of the principles in detail. "When a person dies in any of these lazarettos, unless the physician and surgeon of the Office of Health declare that his death is not owing to any contagious cause, and are very clear in their report, all those who are under quarantine are obliged to recommence it; and this as often as there happens any suspected death." This, and other vexations, such as having no pay, sometimes give rise to mutinies among the crews detained in quarantine. *Howard on Lazarettos, p.* 7.

from its consequences, does not appear to merit farther, consideration.

2. The second kind of quarantine is that performed by ships having no suspected sickness, and bearing clean bills of health; that is, a certificate that there was no suspected sickness at the port from whence they sailed, during their loading, or at the period of their departure. The time is fifteen days, to which must be added that occupied in discharging such parts of their cargoes as consist of enumerated articles [1]. Quarantine, under these circumstances, is performed at different specified ports.

That a ship arriving in England, not only without any sickness, but with a clean bill of health, should even according to the doctrine of contagion, be required to do quarantine for a single minute, seems to me to be a regulation without an object; when we reflect that all ships coming from Turkey perform more than a full quarantine at sea during the passage. Is it not a violation of all common sense, that we,

[1] Ships furnished with clean bills of health shall, as well as the crews and passengers, perform a quarantine of fifteen days from the date of the delivery into the lazaretto, of such part of the cargoes as consist of the goods enumerated in class first and second; or if there be no such cargo, from the arrival of the ships at their appointed stations. O. C. 5th Ap. 1805, s. xii.

who are double the distance from Turkey with some other countries, should do double the quarantine? It appears even more preposterous, in proportion as we are more removed from the focus of the supposed infection, than the quarantine of *eighty days*, which used to be performed at Venice, but is now considered superfluous, even there. "Formerly," says Howard, "when persons who had the plague were brought from the city, (Venice), they were put, for forty days, into a large room in the lazaretto, and afterwards into another apartment, for the same time, before they were discharged [1]."

The period now supposed sufficient for expurgation is forty days from the commencement of the malady; and forty minutes, forty hours, forty weeks, or forty years, would, in point of principle, have been equally justifiable, or equally absurd.

The only danger that can be supposed to exist, under this species of quarantine, is that which is presumed to depend upon the capacity of goods to receive, retain, and communicate infection. This, as shall be presently shewn, amounts to nothing; but this quarantine has the advantage over the two following kinds, that,

[1] Lazarettos, pp. 9 and 12.

with precisely the same risk, it exacts only half the detention.

The following quarantines, although subject to different periods, are all performed upon grounds similar to each other, and similar to the preceding.

3. A ship sailing from Turkey, with *suspected* bills of health, commonly called *touched* patents; that is, when a suspicion of plague exists at the place of her lading or departure, but without suspected sickness on board at the period of her arrival in England, performs quarantine for thirty-four days at Stangate Creek only [1].

4. A vessel sailing without clean bills of health, that is, with a certificate that the plague is understood to be actually existing at the port of her lading or departure, but arriving without suspected sickness in England, performs quarantine for forty-four days *at Stangate Creek only.* This is at present, and has been for a long time, by far the most frequent species of quarantine [2].

[1] "Ships, &c. arriving with *suspected* bills of health, (commonly called *touched* patents, or bills,) shall respectively be subject to ten days less quarantine than those without clean bills of health." O. C. Ap. 5, 1805. s. xli.

[2] Ships without clean bills of health, but having no suspected disease on board, shall perform quarantine at Stan-

5. Goods, enumerated in class first and second, brought to England in ships that have already performed quarantine at any of the lazarettos in the Mediterranean, are directed to perform a farther quarantine of fifteen days [1].

6. A similar quarantine is directed to be performed by goods, the growth, produce, or manufacture of Turkey, &c., coming from any place in Europe, without the Straits, or on the continent of America, where there is not a regular establishment for the performance of quarantine, as if coming directly from the Mediterranean [2], &c.

The last seems to be merely a politico-commercial regulation.

Upon what principle it has been decided that the Scilly Isles are the proper station for ships having *suspected* sickness on board to perform quarantine, or that Stangate Creek is the proper station for ships having no sickness, I am equally at a loss to conjecture.

But, if the occasional detention of a ship, passengers, crew, and cargo, for forty-four days at the Scilly Isles, on account of the sickness of one, or a few persons on board, and who might

gate Creek, and *no where else*. O. C. Ap. 5, 1805, s. xiv. xxxvii. and xxxix.

[1] O. C. Ap. 5, 1805, s. xliii [2] Ibid.

have been at once removed, and the rest liberated, be, as I have shewn, both absurd, and, as far as it operates, mischievous ; what ought we to think of the detention, for the same period of time, of almost the whole commerce of the Levant, without any sickness, and consequently without any assignable motive, at Stangate Creek? For it is obvious that the want of a clean bill of health cannot in any view of reason amount to a motive. There being no sickness, no apprehension can be entertained of infection from persons : for, if the crew of a ship could be supposed to go to sea, their persons being loaded with infection, without the malady having actually made its appearance, either the whole of it would be blown away before they could make one fourth of the passage, or it would remain entirely inoperative, from the absence of that disposition of the air which is considered by the contagionists indispensable to render it efficient.

As of late years very few vessels have arrived from Turkey with clean bills of health, so nearly all the ships in that trade, belonging to the united kingdom, have been obliged to perform quarantine ; that is, to lose one third of their time, without an object, at Stangate Creek. And as it is of constant occurrence, and as the other quarantine, (for *suspected* sickness, at the Scilly Isles) but very rarely happens, this comprehends

almost the whole of the evil. Indeed, it may reasonably be presumed to be alone sufficient to arrest the progress of the trade of this country with the Levant; and but for the destruction of that of France, in consequence of the events of the war, would probably, 'ere this, have diminished it still more. It remains to be seen, whether, upon the revival of the trade of France with Turkey, in the event of the continuance of peace between France and England, this will not actually be the result.

What determines the duration of quarantine is the nature of bills of health.

A bill of health is a document from the consul at the port from which a vessel sails, in any of the countries specified, purporting either that, at the time of her sailing there was no plague, or suspicion of plague; that there were rumours of plague, but no actual plague; or that there was actual plague. The first are called *clean* bills, the second *suspected*, and the last *foul* bills; or, by a courteous circumlocution, being *without clean* bills of health.

Although there be no sickness on board, there does not, upon the supposition of contagion, appear to be any difference in the risk: vessels upon their arrival in England have, as we have seen, quarantines allotted to them of very different periods, according to the specification of the bill of health with which they may be furnished

that is, fifteen, thirty-four, and forty-four days respectively. And if the detention of fifteen days be justly deemed a hardship, where there is not the smallest shadow of risk, how much greater is the hardship of being detained for thirty-four or forty-four days, where the danger is equally chimerical!

Bills of health, then, not being founded upon any probable danger, arising from the existence of actual sickness among the crew, of a nature suspected to be infectious; but upon the supposition of the possibility of an infection (itself not proved) being capable of being communicated at some uncertain period, by some unknown artificer, to some undescribed article of produce or manufacture, regarded, upon some unintelligible principle, as capable of receiving, retaining, and again communicating that infection, upon some unexpected occasion, to living persons, must be regarded as not only evidently absurd in their principle, but almost an insult to the understanding.

Let us examine the practice. A single *accident* (the term used to denote death) from the plague, or the report of an accident occurring in some obscure quarter of the great cities of Smyrna or Constantinople, whilst a ship is loading for England, is deemed sufficient ground to refuse that ship a clean bill of health. (We have seen

that even clean bills of health do not exempt from quarantine.) But even this ground, such as it is, can never be a matter of certainty. There are no means of ascertaining the truth of these reports. The Consuls cannot themselves, nor any of those immediately connected with them, if they were so inclined, enter into personal inquiries, as this might subject them to quarantine, or, as is supposed, to danger of infection. These reports they are, therefore, obliged to take upon trust; and, as they are often fabricated for purposes of commercial speculation, those who give them credit are very liable to be deceived. Indeed, those to whose department it belongs to grant bills of health, whilst they have reason to believe the reports that are in circulation to be fabricated, consider themselves as having no option, but to act upon them. Hence ships may be despatched without clean bills of health, when there is neither plague, nor any suspicious sickness; whilst those who have fabricated the reports upon which they have been refused, may have been transmitting cargoes, by circuitous routes, in order to arrive at our markets before our own can have finished their quarantine; and if this mischief can even be in any degree remedied by protecting regulations, it must still be at the price of additional restrictions upon naviga-

tion and commerce, and enhancing the value of the commodities of the Levant to the general consumer.

At Constantinople there are, at almost all periods of the year, and in almost every year, straggling cases, or reports of cases, of plague; whilst at Smyrna, the periods of its commencement and cessation are more distinctly defined. A town must be free from plague six weeks before clean bills of health can be issued[1]. For these reasons, although clean bills of health are sometimes issued at Smyrna, it is very rarely, especially of late years, that they have been granted at Constantinople. And this may be, in part, the reason why the Smyrna market is better supplied with cargoes for this part of the world; why ships prefer going there to load; and why so few English ships now resort to the Turkish capital.

[1] The reports respecting the cessation, as well as the commencement of plague, are equally subject to uncertainty, even if the principle were correct upon which bills of health are granted. It is singular, that, by writers and travellers, the opinion that plague does not depend upon contagion, has been imputed as an error to the Turks, as connected with their ideas of fatalism; whilst the real error upon this subject has been regarded as demonstrated truth, and its fraudulent origin not observed.

It may also afford a solution, to a certain extent, of the great advantages, under the system of quarantine, which the French Levant trade enjoys over that of Great Britain; advantages which would be much less considerable, if no quarantine were performed in either country.

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It is upon the chimerical notion, as it now appears, that certain goods, wares, and merchandises, therefore called *susceptible*, are capable of receiving, retaining, and again communicating, at some uncertain period, to the living body an infection capable of producing desolation and death amongst mankind; that both bills of health, and quarantines, are founded: for, the occurrence of *suspected disease*, as a ground of quarantine, is so rare, if it ever happen, that, regarded in its effects, it is scarcely worthy of consideration.

There is not, as I am informed, an instance upon record, of a ship being obliged to do quarantine at the Scilly Isles, on account of an actual pestilence on board. Consequently, even according to the doctrines of contagion, quarantine in this country can have no object, but in respect to goods.

But the positive facts which shew that goods do

not propagate infection, are if possible, even more unequivocal than those which relate to persons. From the information which I obtained at Malta, and in other places, I feel myself authorized in concluding, that persons employed in the expurgation of goods in the lazarettos have been even less frequently affected by epidemic diseases, in proportion to their numbers, than some other classes of the community [1]. Nor is it often that persons have been seized with an epidemic, or sporadically from the foul air of packages, whilst packing and unpacking goods, or stowing and unstowing cargoes.

Here, then, the whole of that extraordinary doctrine of articles susceptible of contagion in the first degree, articles susceptible in the second degree, and articles non-susceptible, upon which bills of health, quarantines, and other regulations of plague police have been founded, falls to the ground [2].

And thus all the tales respecting the propagation of infection in epidemic diseases, whether

[1] This fact is acknowledged, in his answers to my queries, by the President of the College of Physicians *(Proto-Medico)* of that island, although a firm believer in contagion.

[2] For the fanciful classification here alluded to, see Order in Council, April 5, 1805, s. xxx. and xxxv.

from the living body or inanimate matter, with which the public have been deluded, or amused, for two hundred and seventy years, are distinctly ascertained to be the offspring of a stratagem.

But granting, for a moment, that the doctrine of contagion were true, that infection may be propagated from persons to goods, and from goods again to persons, what would be the consequences in respect to quarantine? The consequences would be, that the quarantines at present in use would be wholly inadequate to their object; and that *no quarantine could ensure safety, without being interminable!*

It must be quite obvious, that, if infection existed in goods, no quarantine could avail that was not of a period somewhat longer than the greatest duration of the capability of such goods to retain and communicate the infection. Thus, if the plague, as we are told, spread in Paris from rags after having lain several years in an old wall[z], *several years* is the *shortest* period for which rags ought to perform quarantine. But, if infection can exist in rags for several years, what certainty can we have that it may not continue to exist for several ages, or for several centuries? Again, a feather-bed, as we are informed, having communicated infection *at the end of seven*

[z] Theodore Mayerne Conseils contre la Peste, &c. p. 631

*years*[1]; seven years is the *shortest* quarantine that feathers ought to be made to perform. And what guarantee have we of safety even at the end of that period? If feathers be capable of retaining infection for seven years, how shall we ascertain whether they be not capable of retaining it for seventy, or seven hundred years?

The absurdity of these consequences ought, I should think, to be quite conclusive of the argument.

Regarded, as they affect the interests of commerce and navigation, the consequences of this error will be found to diminish its profits, probably, more than two thirds; since, at present, as much time is consumed, in making *one* voyage to the Levant, as, but for the regulations in question, would be required for *three;* not to speak of the imposts, trammels, uncertainties, expences, impositions, and delays, which they otherwise occasion to trade.

Perhaps the best mode of estimating the effect of quarantine upon commerce and navigation, is to compare those of France and England with Turkey at various periods.

Previous to 1669, when there was no quarantine in either country, the Levant trade of England, as we learn from Anderson's useful work upon commerce, was superior to that of France.

[1] Alexander Benedictus, de Peste, c. 3.

From 1669 to 1720, quarantine being established in France, but not in England, our Levant commerce still continued to preponderate.

From 1720 to 1785, quarantine existing in both countries, the trade of England regularly declined; so that, from 1760 to 1785, the average annual exports were not quite equal to one-fourth of those of the twenty years preceding 1720, nor the imports to one-half[1].

During the same period the trade of France with Turkey regularly increased; so that its exports, in 1787, were to those of England, in 1785, as fifteen and a half; and its imports as ten and a half to one[2].

The state of our Levant commerce, from the breaking out of the war, a few years afterwards, to the present period, as there was no competition, can afford no data.

As the remedies which were attempted to be applied to these evils all failed of success, we may presume, that the causes to which they had been imputed were not the right ones.

Under the persuasion that our inferiority might depend upon the limitations under which our Levant trade was conducted, although these limi-

[1] And. Com. Vol. IV. passim.

[2] In 1787, the exports and imports of France to Turkey amounted each to about a million sterling.—*And. Com.* Vol. IV. p. 657.

tations existed in an equal or greater degree when our commerce surpassed that of France, an act of parliament was passed in 1754, by which every subject of Great Britain, desiring to become a member of the Turkey Company, was directed to be admitted, within thirty days after making such request, paying for such admission twenty pounds for the use of the Company, and no more [1].

Notwithstanding this enlargement, the trade continued to decline from 1754 to 1785, more rapidly than before; and as there appears to be no reason to doubt that this state of decay would have continued progressive, it is more than probable that, but for the intervention of the French war of the Revolution, and the destruction of the French commerce of the Levant, ours would, ere this, have been wholly annihilated.

Amongst the palliatives resorted to in mitigation of this evil, was a law, passed in 1759, to prohibit British subjects from exporting French broad-cloth to Turkey, and from importing raw silk from Leghorn into Great Britain [2].

A duty was also laid, in 1781, upon cotton and cotton wool, imported into this kingdom in foreign ships or vessels, during the then existing hostilities [3].

[1] And. Com. Vol. III. p. 292, 3.
[2] Ibid. p. 317, 8. [3] Ibid. Vol. IV. p. 398.

But these protecting laws, as they did not touch the main cause of the evil, were, as might be expected, wholly unavailing.

From the moment of the establishment of the quarantine laws in England, our Levant commerce began and continued to decline, until the Turkey trade of France also fell into our hands. This coincidence clearly indicates these laws as the main cause of the evils. It is both so obvious and so adequate, that there can be no necessity to look for any other; and if we do not, by its removal, apply the proper remedy, it is not difficult to foresee, that as France is about to resume her rivalry, perhaps with more advantages than before, it will not be long before she regains her superiority.

Although there can be no doubt that quarantine operates injuriously to commerce and navigation in France, and in every other country in which it has been adopted, as well as in England; yet it must be observed, that neither the inconvenience nor the expence of doing quarantine at Marseilles, when it is even of the same duration, can be near so great as at the Isles of Scilly, or in Stangate Creek, or in Milford Haven. And hence the advantages enjoyed by France, from the situation of Marseilles, when there were no quarantines in either country, must be now

greatly inhanced by the different operation of those establishments in each.

Under the present system, it is evident, that, from proximity merely, vessels may go from Marseilles to Smyrna, embracing the intervals in which that place is known to be free from plague, and from rumours of plague; and after having supplied themselves with all that the demand requires of the commodities of the Levant, may return at seasonable periods, subject only for the most part, to the short detention required under clean bills of health, whilst by far the greatest number of our vessels, not having the same facilities of embracing the most favourable seasons, are placed under the circumstances which subject them to the performance of the long quarantine of forty-four days.

This constitutes a very great difference in respect to the injury which the trade of each country suffers from those regulations. But there is a further circumstance, which is also much against us in this comparison. Marseilles being the most conveniently situated point in France for the centre of Levant commerce, the merchandize of these countries, after suffering, generally, no farther detention at that port, than the period of the shortest quarantine, are promptly transmitted to all the other parts of the kingdom; whilst

ours, by being consigned to Stangate Creek, is doomed, after having performed the longest period of quarantine, to the farther detention of a circuitous navigation, arising from the inappropriate situation, excepting for the port of London, of that place.

With respect to the comparative disadvantages to which our Levant commerce is exposed by the quarantine laws, and of their positive mischiefs, which are both great and numerous, not intending to enter technically into the subject, I shall only farther observe, without insisting upon incidental expenses, the tear and wear of ships, the wages of crews, or the waste or destruction of cargoes, that the *detention merely* must either have the effect of diminishing the profits, on the commerce and navigation to the Levant, by one-third, or of inhancing in the same ratio, the price of the commodities of that country to the general consumer, since, but for this detention, vessels would make at least three voyages, where they now make only two.

The severity with which, even under the least unfavourable circumstances, the effects of quarantine are felt by the commercial community, is well evinced by the following circumstance. The Council of Commerce, established by the French King in 1700, presented a memorial to that King's council, in which, among other

things, they represent that, "It would be more advantageous for France to permit her ports on the ocean to carry on this trade directly to the Levant, without being obliged, ever since the year 1669, to unlade at Marseilles, on their return, *under pretence* of preventing their bringing in the plague; *which has obliged them to relinquish that trade entirely* [1].

How much more heavily the disadvantages of these regulations have weighed upon the commerce of England, where, from its greater distance from the seat of the supposed infection, the pretext is still more unreasonable, cannot be doubted.

In proportion as these regulations affect commerce, it is obvious how they must affect the interests of the general consumer, and the public revenue.

Of the evils which have been occasioned by this doctrine, to naval and military expeditions, and the operations of armies, some specimens are given in the Preliminary Discourse.

Of the manner, in which it affects individual intercourse, and the intercourse of nations, some idea may be formed, by supposing a traveller on the point of setting out from Constantinople, or some other part of the Turkish dominions, and

[1] And. Com. Vol. III. p. 7.

considering which way he should proceed, in order to meet with the least possible detention.

He can turn his face to no part of Europe, without the prospect of a tedious quarantine, whether there exist, or not, a pestilence, at the period of his departure; and his only considerations are, where he is likely to suffer the least, from detention, bad air, bad treatment, and filth.

Independently of human lives, in a view of political economy, a saving might be affected, of the sums which are now annually expended, on the quarantine, and other police establishments consequent upon this belief; as well as of the amount of the indirect diminutions of revenue, which the restrictions flowing from it occasion.

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Previous to 1800, ships without clean bills of health were not admitted to entry in these kingdoms, unless after having performed quarantine in one of the principal foreign lazarettos (we had then none of our own,) in the Mediterranean. This regulation has existed since the first introduction of quarantine into this country, in 1720. From that period to the year 1800, quarantine, with clean bills of health, was performed in England in old ships of war, called floating lazarets; and the expense born by his Majesty's government.

In 1800, it being deemed expedient to remove the restrictions mentioned, an act (39 and 40 Geo. III. c. 80,) was passed, authorizing ships to enter without clean bills of health; appropriating the sum of £.65,000 from the consolidated fund, for the establishment of a land lazaret at Chetney Hill, and imposing a tonnage duty upon the shipping employed in the Levant trade, to reimburse the cost of Chetney Hill, and to provide for the future expences of quarantine, as well afloat as on shore [1].

In 1805, this sum having been found insufficient, another act was passed, to make further provision, and to confirm the act of 1800 in all its conditions.

Under the authority of these acts between £250,000 and £300,000 have, since 1800, been

[1] Experience of the comparative disadvantages arising to our commerce from the performance of quarantine, with foul bills of health, in the Mediterranean, and, perhaps, the difficulty of procuring access to the foreign lazarettos in war time, after France had extended her conquests, pointed out the expediency of transferring them to England: and, were these institutions useful, this measure would certainly have been an improvement. Its propriety was pointed out several years before it was carried into effect, in an able letter, addressed by the merchants of Smyrna, to Mr. Howard, and inserted at page 26 of his Account of the Principal Lazarettos of Europe. But my conclusions being admitted, this transfer can only be regarded as the improvement of an error.

levied upon the trade of the Levant. The land lazaret remains still unfinished. And, after an expense has been incurred of between one and two hundred thousand pounds, in the purchase of lands, and the erection of works, doubts have even arisen respecting the healthiness of the situation.

Here, then, is a direct expense of from £16,000 to £.20,000 a year entailed on the public for an object that is not simply useless, but injurious; for whether it be paid in the first instance by a duty on the Levant trade, or otherwise, the amount always comes ultimately from the pockets of the consumer.

This is an ascertained expenditure. But we know not how much has been advanced out of the consolidated or other funds, to support the quarantine establishments in this country, beyond what has been reimbursed by the tonnage duty levied upon the trade.

This is properly a subject of official enquiry and the truth may be easily ascertained.

To the amount expended upon quarantine establishments at home, we have now to add that which is required for those of Malta, Gibraltar, and such as have recently devolved to us in the Ionian islands. The whole united cannot fall short of a hundred thousand pounds a year, and may possibly exceed two.

Were the expense indeed twenty times as great, there is no doubt that if these establishments could be shewn to be unequivocally useful, it ought and would be cheerfully born. But as it has, I think, been proved, that the object for which they were instituted does not exist, and that they are otherwise highly injurious, it follows that if the expense were only one twentieth part of the actual amount, it ought to be forthwith discontinued [1].

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From the whole of this discussion, it must have become obvious, why the means, which have been adopted in this country, for investigating the subject of quarantine, could not have been attended with any success. The committees, or boards, appointed for this purpose, having invariably set out by taking for granted that, which ought to have been the first thing submitted to rigid examination, viz. the existence of contagion as the cause of epidemic diseases generally; have thought that the only question, which they were required to decide, respected the existence of this cause in some

[1] These institutions have been brought, according to the existing ideas, to the highest perfection, at Odessa, under the government of the Duke de Richlieu; and at Malta, by Lieutenant-General Sir Thomas Maitland. But they never have been, and never will be, able to prevent the introduction, or spreading of pestilence, when the circumstances occur that would otherwise produce it.

particular epidemic. The reason is also obvious, why, under the belief of the doctrine of contagion generally, it was difficult to arrive at the truth, in respect to particular pestilences; *i. e.* to distinguish, under such a prepossession, between the effects of a specific contagion, and of a noxious atmosphere. Hence, the boards, which have from time to time, been appointed for such purposes, have either separated without coming to any definite conclusions, or have formed conclusions that were both erroneous, and pernicious. But the delusion is now dissipated. And, in this country at least, there can be no grounds, upon the presumption of any insurmountable popular prejudices, for hesitation, concerning the immediate renunciation of the doctrine of contagion, as the cause of epidemic diseases; or the immediate abolition of all the costly institutions of police, which have arisen out of that preposterous belief.

It is not a little remarkable, that, at the very moment, in which I was employed in prosecuting an investigation, the result of which is to shew that these institutions, where they already exist, should be abolished as pernicious, the philanthropic German Physician, Count Harrach [1], should, with precisely similar views,

[1] I have seldom been more highly gratified, than, in witnessing the active benevolence of this efficient friend to

have been occupied in recommending to the Congress of Sovereigns at Vienna, to adopt measures for enforcing their extension to countries, in which they have not yet been received, especially to the Othoman empire. If the doctrine of contagion were correct; and if police regulations were capable of alleviating the calamities of pestilential maladies; it would, indeed, be incumbent upon all nations to unite their influence in order to enforce the general adoption of this system. But, if the contrary, as I contend, be true; it is equally incumbent upon those, who already possess those establishments, to enact, after due enquiry, their immediate abolition. And the means are here offered, by which they may come to a satisfactory decision.

the human species, who employs his time, an ample fortune, and great mental energies, almost exclusively in alleviating the miseries of his fellow-creatures. A mind, like his, is, indeed, abundantly rewarded, in the improving looks, the smiles, the thanks, the blessings, of the many unfortunate beings, whose diseases his skill has cured; whose wants his purse has relieved; and whom his counsels and his influence have extricated from embarrassment, or rescued from oppression. To many, it would alone repay a journey, from London to Vienna, to see the morning levy of Count Harrach.

END OF VOL. I.

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